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THE
LOGIC OF CHANCE.

THE
LOGIC OF CHANCE

AN ESSAY

ON THE FOUNDATIONS AND PROVINCE OF
THE THEORY OF PROBABILITY,

WITH ESPECIAL REFERENCE TO ITS LOGICAL BEARINGS
AND ITS APPLICATION TO

MORAL AND SOCIAL SCIENCE, AND TO STATISTICS,

BY

JOHN VENN, Sc.D., F.R.S.,

FELLOW AND LECTURER IN THE MORAL SCIENCES, GONVILLE AND CAIUS COLLEGE,
CAMBRIDGE.

LATE EXAMINER IN LOGIC AND MORAL PHILOSOPHY IN THE
UNIVERSITY OF LONDON.

> "So careful of the type she seems
> So careless of the single life."

THIRD EDITION, RE-WRITTEN AND ENLARGED.

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1888

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PREFACE TO FIRST EDITION.

ANY work on Probability by a Cambridge man will be so likely to have its scope and its general treatment of the subject prejudged, that it may be well to state at the outset that the following Essay is in no sense mathematical. Not only, to quote a common but often delusive assurance, will 'no knowledge of mathematics beyond the simple rules of Arithmetic' be required to understand these pages, but it is not intended that any such knowledge should be acquired by the process of reading them. Of the two or three occasions on which algebraical formulæ occur they will not be found to form any essential part of the text.

The science of Probability occupies at present a somewhat anomalous position. It is impossible, I think, not to observe in it some of the marks and consequent disadvantages of a *sectional* study. By a small body of ardent students it has been cultivated with great assiduity, and the results they have obtained will always be reckoned among the most extraordinary products of mathematical genius. But by the general body of thinking men its principles seem to be regarded with indifference or suspicion. Such persons may admire the ingenuity displayed, and be struck with the profundity of many of the calculations, but there seems to them, if I may so express it, an *unreality* about the whole treatment of the subject. To many persons the mention of Probability suggests little else than the notion of a set of rules, very ingenious and profound rules no doubt, with which mathematicians amuse themselves by setting and solving puzzles.

It must be admitted that some ground has been given for such an opinion. The examples commonly selected by writers on the subject, though very well adapted to illustrate its rules, are for the most part of a special and peculiar character, such as those relating to dice and cards. When they have searched for illustrations drawn from the practical business of life, they have very generally, but unfortunately, hit upon just the sort of instances which, as I shall endeavour to show hereafter, are among the very worst that could be chosen for the purpose. It is scarcely possible for any unprejudiced person to read what has been written about the credibility of witnesses by eminent writers, without his experiencing an invincible distrust of the principles which they adopt. To say that the rules of evidence sometimes given by such writers are broken in practice, would scarcely be correct; for the rules are of such a kind as generally to defy any attempt to appeal to them in practice.

This supposed want of harmony between Probability and other branches of Philosophy is perfectly erroneous. It arises from the belief that Probability is a branch of mathematics trying to intrude itself on to ground which does not altogether belong to it. I shall endeavour to show that this belief is unfounded. To answer correctly the sort of questions to which the science introduces us does generally demand some knowledge of mathematics, often a great knowledge, but the discussion of the fundamental principles on which the rules are based does not necessarily require any such ation. Questions might arise in other sciences, in y, for example, which could only be answered by the arithmetical calculations. In such a case any one admit that the arithmetic was extraneous and acci- However many questions of this kind there might e, those persons who do not care to work out special for themselves might still have an accurate know- f the principles of the science, and even considerable ıtance with the details of it. The same holds true in ility; its connection with mathematics, though cer- far closer than that of most other sciences, is still of the same kind. It is principally when we wish to ut results for ourselves that mathematical knowledge ired; without such knowledge the student may still firm grasp of the principles and even see his way to of the derivative results.

e opinion that Probability, instead of being a branch of ıeral science of evidence which happens to make much mathematics, *is* a portion of mathematics, erroneous as ıas yet been very disadvantageous to the science in ways. Students of Philosophy in general have thence ed a prejudice against Probability, which has for the art deterred them from examining it. As soon as a comes to be considered 'mathematical' its claims enerally, by the mass of readers, to be either on the nd scouted or at least courteously rejected, or on the to be blindly accepted with all their assumed conse- s. Of impartial and liberal criticism it obtains little ıing.

e consequences of this state of things have been, I disastrous to the students themselves of Probability. ence can safely be abandoned entirely to its own devo- Its details of course can only be studied by those who make it their special occupation, but its general princi are sure to be cramped if it is not exposed occasionall the free criticism of those whose main culture has be a more general character. Probability has been very abandoned to mathematicians, who as mathematicians generally been unwilling to treat it thoroughly. They worked out its results, it is true, with wonderful acute and the greatest ingenuity has been shown in solving va problems that arose, and deducing subordinate rules. this was all that they could in fairness be expected t Any subject which has been discussed by such me Laplace and Poisson, and on which they have exhauste their powers of analysis, could not fail to be profou treated, so far as it fell within their province. But from province the real principles of the science have gene been excluded, or so meagrely discussed that they had b have been omitted altogether. Treating the subject as thematicians such writers have naturally taken it up at point where their mathematics would best come into and that of course has not been at the foundations. In works of most writers upon the subject we should searc vain for anything like a critical discussion of the fu mental principles upon which its rules rest, the cla enquiries to which it is most properly applicable, or relation it bears to Logic and the general rules of indu evidence.

This want of precision as to ultimate principles is fectly compatible here, as it is in the departments of M and Politics, with a general agreement on processes results. But it is, to say the least, unphilosophical, denotes a state of things in which positive error is al liable to arise whenever the process of controversy forc to appeal to the foundations of the science.

th regard to the remarks in the last few paragraphs, ient exceptions must be made in the case of two recent at least[1]. The first of these is Professor de Morgan's *l Logic.* He has there given an investigation into the tions of Probability as conceived by him, and nothing more complete and precise than his statement of les, and his deductions from them. If I could at all with these principles there would have been no neces- r the following essay, as I could not hope to add ng to their foundation, and should be far indeed from ıg his lucid statement of them. But in his scheme ility is regarded very much from the Conceptualist of view; as stated in the preface, he considers that ility is concerned with formal inferences in which the es are entertained with a conviction short of absolute ty. With this view I cannot agree. As I have entered iticism of some points of his scheme in one of the ng chapters, and shall have occasion frequently to refer work, I need say no more about it here. The other o which I refer is the profound *Laws of Thought* of e Professor Boole, to which somewhat similar remarks ı part be applied. Owing however to his peculiar ent of the subject, I have scarcely anywhere come ntact with any of his expressed opinions.

ə view of the province of Probability adopted in this liffers so radically from that of most other writers on ject, and especially from that of those just referred to, have thought it better, as regards details, to avoid all m of the opinions of others, except where conflict was

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[1] ı here speaking, of course, of ly who have expressly treated ındations of the science. Mr er's admirable work on the *History of the Theory of Probability* being, as the name denotes, mainly historical, such enquiries have not directly fallen within his province.

THE
LOGIC OF CHANCE.

PREFACE TO SECOND EDITION.

principal reason for designating this volume a second ion consists in the fact that the greater portion of what be termed the first edition is incorporated into it. Be- various omissions (principally where the former treat- t has since seemed to me needlessly prolix), I have added matter, not much inferior in amount to the whole of original work. In addition, moreover, to these altera- in the matter, the general arrangement of the subject regards the successive chapters has been completely nged; the former arrangement having been (as it now ns to me) justly objected to as deficient and awkward in hod.

After saying this, it ought to be explained whether any nge of general view or results will be found in the present tment.

The general view of Probability adopted is quite un- nged, further reading and reflection having only confirmed in the conviction that this is the soundest and most ful way of regarding the subject. It is the more ne- ry to say this, as to a cursory reader it might seem otherwise; owing to my having endeavoured to avoid t needlessly polemical tone which, as is often the case w those who are making their first essay in writing upon a subject, was doubtless too prominent in the former editi I have not thought it necessary, of course, except in one two cases, to indicate points of detail which it has seem necessary to correct.

A number of new discussions have been introduced up topics which were but little or not at all treated before. T principal of these refer to the nature and physical origi Laws of Error (Ch. II.); the general view of Logic, and c sequently of Probability, termed the Material view, adop here (Ch. X.); a brief history and criticism of the vari opinions held on the subject of Modality (Ch. XII.); logical principles underlying the method of Least Squ (Ch. XIII.); and the practices of Insurance and Gambl so far as the principles involved in them are concer (Ch. XV.). The Chapter on the Credibility of Extraordi Stories is also mainly new; this was the portion of the for work which has since seemed to me the least satisfac but owing to the extreme intricacy of the subject I am from feeling thoroughly satisfied with it even now.

I have again to thank several friends for the assist they have so kindly afforded. Amongst these I must pr nently mention Mr C. J. Monro, late fellow of Trinity. I only the truth to say that I have derived more assistance f his suggestions and criticisms than has been consciously tained from all other external sources together. Much of icism has been given privately in letters, and notes on proof-sheets; but one of the most elaborate of his discus- ns of the subject was communicated to the Cambridge ilosophical Society some years ago; as it was not published, wever, I am unfortunately unable to refer the reader to it. ught to add that he is not in any way committed to any my opinions upon the subject, from some of which in fact more or less dissents. I am also much indebted to r J. W. L. Glaisher, also of Trinity College, for many hints d references to various publications upon the subject of ast Squares, and for careful criticism (given in the midst much other labour) of the chapter in which that subject is ated.

I need not add that, like every one else who has had to cuss the subject of Probability during the last ten years, have made constant use of Mr Todhunter's History.

I may take this opportunity of adding that a considerable rtion of the tenth chapter has recently appeared in the nuary number of *Mind*, and that the substance of several apters, especially in the more logical parts, has formed part my ordinary lectures in Cambridge; the foundation and gical treatment of Probability being now expressly included the Schedule of Subjects for the Moral Sciences Tripos.

March, 1876.

PREFACE TO THIRD EDITION

present edition has been revised throughout, and in fact itten. Three chapters are new, viz. the fifth (On the eption of Randomness) and the eighteenth and nine- th (On the nature, and on the employment, of Averages). eighth, tenth, eleventh, and fifteenth chapters have been st, and much new matter added, and numerous altera- s made in the remaining portions[1]. On the other hand e chapters of the last edition have been nearly or rely omitted.

These alterations do not imply any appreciable change of v on my part as to the foundations and province of bability. Some of them are of course due to the necessary nges involved in the attempt to write up to date upon a ject which has not been stationary during the last eleven rs. For instance the greatly increased interest now en in what may be called the Theory of Statistics has lered it desirable to go much more fully into the Nature treatment of Laws of Error. The omissions are mainly due to a wish to avoid increasing the bulk of this volum more than is actually necessary, and to a feeling that th portions treating specially of Inductive Logic (which occupied some space in the last edition) would be mor suitable to a regular work on that subject. I am at presen engaged on such a work.

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[1] have indicated the new chapters and sections by printing them in s in the Table of Contents.

The publications which I have had occasion to notic have mostly appeared in various scientific journals. Th principal authors of these have been Mr F. Galton an Mr F. Y. Edgeworth: to the latter of whom I am als personally much obliged for many discussions, oral an written, and for his kindness in looking through the pro sheets. His published articles are too numerous for separat mention here, but I may say generally, in addition to th obligations specially noticed, that I have been considerab indebted to them in writing the last two chapters. T authors of works of a somewhat more substantial charact viz. Prof. Lexis and Von Kries, only came under my noti unfortunately after this work was already in the printe hands. With the latter of these authors I find myself closer agreement than with most others, in respect of general conception and treatment of Probability.

December, 1887.

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[1] hapters and sections which are nearly or entirely new are printed in *italics*.

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THE LOGIC OF CHANCE.

CHAPTER I.

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1. It is sometimes not easy to give a clear definition of a ence at the outset, so as to set its scope and province before reader in a few words. In the case of those sciences ich are more immediately and directly concerned with at are termed objects, rather than with what are termed cesses, this difficulty is not indeed so serious. If the der is already familiar with the objects, a simple reference them will give him a tolerably accurate idea of the ection and nature of his studies. Even if he be not iliar with them, they will still be often to some extent nected and associated in his mind by a name, and mere utterance of the name may thus convey a fair ount of preliminary information. This is more or less case with many of the natural sciences; we can often the reader beforehand exactly what he is going to study. when a science is concerned, not so much with objects ctly, as with processes and laws, or when it takes for the ject of its enquiry some comparatively obscure feature n from phenomena which have little or nothing else in non, the difficulty of giving preliminary information nes greater. Recognized classes of objects have then

to be disregarded and even broken up, and an entirely no
arrangement of the objects to be made. In such cases it
the study of the science that first gives the science its uni
for till it is studied the objects with which it is concern
were probably never thought of together. Here a definit
cannot be given at the outset, and the process of obtainin
may become by comparison somewhat laborious.

The science of Probability, at least on the view taken
it in the following pages, is of this latter description. T
reader who is at present unacquainted with the scie
cannot be at once informed of its scope by a reference
objects with which he is already familiar. He will h
to be taken in hand, as it were, and some little t
and trouble will have to be expended in directing
attention to our subject-matter before he can be expecte
know it. To do this will be our first task.

§ 2. In studying Nature, in any form, we are continu
coming into possession of information which we sum u
general propositions. Now in very many cases these gen
propositions are neither more nor less certain and accu
than the details which they embrace and of which they
composed. We are assuming at present that the truth
these generalizations is not disputed; as a matter of
they may rest on weak evidence, or they may be uncer
from their being widely extended by induction; wha
meant is, that when we resolve them into their compo
parts we have precisely the same assurance of the trut
the details as we have of that of the whole. When I kn
for instance, that all cows ruminate, I feel just as cer
that any particular cow or cows ruminate as that the w
class does. I may be right or wrong in my original st
ment, and I may have obtained it by any conceivable m
in which truths can be obtained; but whatever the val

e general proposition may be, that of the particulars is
ither greater nor less. The process of inferring the par-
ular from the general is not accompanied by the slightest
minution of certainty. If one of these 'immediate infer-
ces' is justified at all, it will be equally right in every
se.

But it is by no means necessary that this characteristic
ould exist in all cases. There is a class of immediate in-
rences, almost unrecognized indeed in logic, but constantly
wn in practice, of which the characteristic is, that as they
crease in particularity they diminish in certainty. Let me
ume that I am told that *some* cows ruminate; I cannot
er logically from this that any particular cow does so,
ough I should feel some way removed from absolute dis-
lief, or even indifference to assent, upon the subject; but
I saw a herd of cows I should feel more sure that some of
em were ruminant than I did of the single cow, and my
urance would increase with the numbers of the herd about
ich I had to form an opinion. Here then we have a class
things as to the individuals of which we feel quite in
certainty, whilst as we embrace larger numbers in our
ertions we attach greater weight to our inferences. It is
th such classes of things and such inferences that the
ence of Probability is concerned.

§ 3. In the foregoing remarks, which are intended to
purely preliminary, we have not been able altogether to
oid some reference to a subjective element, viz. the degree
our certainty or belief about the things which we are
posed to contemplate. The reader may be aware that
some writers this element is regarded as the subject-
er of the science. Hence it will have to be discussed
future chapter. As however I do not agree with the
ion of the writers just mentioned, at least as regards

treating this element as one of primary importance, no further allusion will be made to it here, but we will pass on at once to a more minute investigation of that distinctive characteristic of certain classes of things which was intro duced to notice in the last section.

In these classes of things, which are those with whic Probability is concerned, the fundamental conception whic the reader has to fix in his mind as clearly as possible, is, I take it, that of a series. But it is a series of a peculiar kind one of which no better compendious description can be give than that which is contained in the statement that it co bines individual irregularity with aggregate regularity. Th is a statement which will probably need some explanatio Let us recur to an example of the kind already alluded t selecting one which shall be in accordance with experien Some children will not live to thirty. Now if this prop sition is to be regarded as a purely indefinite or, as it wou be termed in logic, 'particular' proposition, no doubt t notion of a series does not obviously present itself in co nection with it. It contains a statement about a certa unknown proportion of the whole, and that is all. But it not with these purely indefinite propositions that we s be concerned. Let us suppose the statement, on the co trary, to be of a numerical character, and to refer to a giv proportion of the whole, and we shall then find it difficult exclude the notion of a series. We shall find it, I thi impossible to do so as soon as we set before us the aim obtaining accurate, or even moderately correct inferen What, for instance, is the meaning of the statement t two new-born children in three fail to attain the age sixty-three? It certainly does not declare that in any gi batch of, say, thirty, we shall find just twenty that f whatever might be the strict meaning of the words, t

ıot the import of the statement. It rather contemplates
r examination of a large number, of a long succession
instances, and states that in such a succession we shall
ıd a numerical proportion, not indeed fixed and accurate at
st, but which tends in the long run to become so. In
'ery kind of example with which we shall be concerned we
ıall find this reference to a large number or succession
objects, or, as we shall term it, *series* of them.

A few additional examples may serve to make this plain.
Let us suppose that we toss up a penny a great many
mes; the results of the successive throws may be conceived
form a series. The separate throws of this series seem to
cur in utter disorder; it is this disorder which causes our
certainty about them. Sometimes head comes, sometimes
il comes; sometimes there is a repetition of the same face,
metimes not. So long as we confine our observation to a
w throws at a time, the series seems to be simply chaotic.
ıt when we consider the result of a long succession we find
marked distinction; a kind of order begins gradually to
ıerge, and at last assumes a distinct and striking aspect.
e find in this case that the heads and tails occur in about
ual numbers, that similar repetitions of different faces do
also, and so on. In a word, notwithstanding the individual
order, an aggregate order begins to prevail. So again if
are examining the length of human life, the different lives
ich fall under our notice compose a series presenting the
ne features. The length of a single life is familiarly un-
rtain, but the average duration of a batch of lives is be-
ming in an almost equal degree familiarly certain. The
ger the number we take out of any mixed crowd, the
rer become the symptoms of order, the more nearly will
average length of each selected class be the same.
ese few cases will serve as simple examples of a property

of things which can be traced almost everywher greater or less extent, throughout the whole field of perience. Fires, shipwrecks, yields of harvest, birth riages, suicides; it scarcely seems to matter what feat single out for observation[1]. The irregularity of the instances diminishes when we take a large number, last seems for all practical purposes to disappear.

In speaking of the effect of the average in thus di ing the irregularities which present themselves in the the attention of the student must be prominently dire the point, that it is not the *absolute* but the *relative* larities which thus tend to diminish without limit. idea will be familiar enough to the mathematician, others it may require some reflection in order to g clearly. The absolute divergences and irregularities from diminishing, show a disposition to increase, and may be) without limit, though their relative importanc a corresponding disposition to diminish without limit. in the case of tossing a penny, if we take a few thro ten, it is decidedly unlikely that there should be a di of six between the numbers of heads and tails; that

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[1] The following statistics will give a fair idea of the wide range of experience over which such regularity is found to exist: "As illustrations of equal amounts of fluctuation from totally dissimilar causes, take the deaths in the West district of London in seven years (fluctuation 13·66), and offences against the person (fluctuation 13·61); or deaths from apoplexy (fluctuation 5·54), and offences against property, without violence (fluctuation 5·48); or students registered at the College of Surgeons (fluctuation 1·85), and the of pounds of manufacture taken for home consumption tion 1·89); or out-door pau tuation 3·45) and tonnage o vessels entered in ballast (fl 3·43), &c." [Extracted fro in the Journal of the Stati ciety, by Mr Guy, March, 1 'fluctuation' here given is sure of the amount of irr that is of departure from the estimated in a way which described hereafter.]

e should be as many as eight heads and therefore as few
wo tails, or *vice versâ*. But take a thousand throws, and
ecomes in turn exceedingly likely that there should be
nuch as, or more than, a difference of six between the
ective numbers. On the other hand the *proportion* of
ds to tails in the case of the thousand throws will be very
ch nearer to unity, in most cases, than when we only took
. In other words, the longer a game of chance continues
larger are the spells and runs of luck in themselves, but
less their relative proportions to the whole amounts
olved.

§ 4. In speaking as above of events or things as to the
ails of which we know little or nothing, it is not of course
lied that our ignorance about them is complete and uni-
sal, or, what comes to the same thing, that irregularity
y be observed in all their qualities. All that is meant is
t there are *some* qualities or marks in them, the existence
which we are not able to predicate with certainty in the
ividuals. With regard to all their other qualities there
y be the utmost uniformity, and consequently the most
plete certainty. The irregularity in the length of human
is notorious, but no one doubts the existence of such
ans as a heart and brains in any person whom he happens
eet. And even in the qualities in which the irregularity
bserved, there are often, indeed generally, positive limits
hin which it will be found to be confined. No person,
instance, can calculate what may be the length of any
ticular life, but we feel perfectly certain that it will not
tch out to 150 years. The irregularity of the individual
ances is only shown in certain respects, as e.g. the length
he life, and even in these respects it has its limits. The
e remark will apply to most of the other examples with
ch we shall be concerned. The disorder in fact is not

universal and unlimited, it only prevails in certain directi and up to certain points.

§ 5. In speaking as above of a series, it will hardly necessary to point out that we do not imply that the obje themselves which compose the series must occur successiv in time; the series may be formed simply by their comi in succession under our notice, which as a matter of f they may do in any order whatever. A register of mortal for instance, may be made up of deaths which took pl simultaneously or successively; or, we might if we plea arrange the deaths in an order quite distinct from eithe these. This is entirely a matter of indifference; in all th cases the series, for any purposes which we need take i account, may be regarded as being of precisely the same scription. The objects, be it remembered, are given to u nature; the order under which we view them is our own vate arrangement. This is mentioned here simply by wa caution, the meaning of this assertion will become more p in the sequel.

I am aware that the word 'series' in the application which it is used here is liable to some misconstruction, b cannot find any better word, or indeed any as suitable in respects. As remarked above, the events need not ne sarily have occurred in a regular sequence of time, tho they often will have done so. In many cases (for inst the throws of a penny or a die) they really do occur in cession; in other cases (for instance, the heights of me the duration of their lives), whatever may have been order of their actual occurrence, they are commonly bro under our notice in succession by being arranged in stati tables. In all cases alike our processes of inference in the necessity of examining one after another of the me which compose the group, or at least of being prepared s, if we are to be in a position to justify our inferences. e force of these considerations will come out in the course the investigation in Chapter VI.

The late Leslie Ellis[1] has expressed what seems to me ubstantially similar view in terms of genus and species, tead of speaking of a series. He says, “When individual es are considered, we have no conviction that the ratios of quency of occurrence depend on the circumstances common all the trials. On the contrary, we recognize in the de- mining circumstances of their occurrence an extraneous ment, an element, that is, extraneous to the idea of the us and species. Contingency and limitation come in (so peak) together; and both alike disappear when we con- r the genus in its entirety, or (which is the same thing) what may be called an ideal and practically impossible ization of all which it potentially contains. If this be nted, it seems to follow that the fundamental principle he Theory of Probabilities may be regarded as included the following statement,—The conception of a genus lies that of numerical relations among the species sub- inated to it.” As remarked above, this appears a sub- tially similar doctrine to that explained in this chapter, I do not think that the terms genus and species are by means so well fitted to bring out the conception of a dency or limit as when we speak of a series, and I there- much prefer the latter expression.

§ 6. The reader will now have in his mind the conception series or group of things or events, about the individuals which we know but little, at least in certain respects, t we find a continually increasing uniformity as we larger numbers under our notice. This is definite o point out tolerably clearly the kind of thin h we have to deal, but it is not sufficiently defini ses of accurate thought. We must therefore a omewhat closer analysis.

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[1] ansactions of the Cambridge ophical Society, Vol. ix. p. 605. Reprinted in the collected edition of his writings, p. 50.

are certain phrases so commonly adopted as t ome part of the technical vocabulary of the sub h as an ‘event’ and the ‘way in which it ca Thus the act of throwing a penny would be calle , and the fact of its giving head or tail would b e way in which the event happened. If we wer g tables of mortality, the former term would de mere fact of death, the latter the age at whic ed, or the way in which it was brought abou ver else in it might be the particular circumstanc scussion. This phraseology is very convenient, an n be made use of in this work, but without expl may lead to confusion. For in many cases th which the event happens is of such great relati ce, that according as it happens in one way the event would have a different name; in oth would not in the two cases be nominally the sam The phrase therefore will have to be considerab . before it will conveniently cover all the cases e may have to apply it. If for instance we we ating a series of human beings, male and fema sound odd to call their humanity an event, a the way in which the event happened.

e recur however to any of the classes of obje eferred to, we may see our path towards obtaini ccurate conception of what we want. It will ea that in every one of them there is a mixture y and dissimilarity; there is a series of eve ave a certain number of features or attributes

—without this they would not be classed together.
re is also a distinction existing amongst them; a
number of other attributes are to be found in some
e not to be found in others. In other words, the
uals which form the series are compound, each being
up of a collection of things or attributes; some of
things exist in all the members of the series, others
ound in some only. So far there is nothing peculiar
ie science of Probability; that in which the distinctive
acteristic consists is this;—that the occasional attri-
es, as distinguished from the permanent, are found on
xtended examination to tend to exist *in a certain definite*
portion of the whole number of cases. We cannot tell in
given instance whether they will be found or not, but
we go on examining more cases we find a growing uni-
ity. We find that the proportion of instances in which
are found to instances in which they are wanting, is
dually subject to less and less comparative variation, and
roaches continually towards some apparently fixed value.
The above is the most comprehensive form of description;
matter of fact the groups will in many cases take a
simpler form; they may appear, e.g. simply as a suc-
ion of things of the same kind, say human beings, with
ithout an occasional attribute, say that of being left-
ded. We are using the word attribute, of course, in its
st sense, intending it to include every distinctive feature
can be observed in a thing, from essential qualities
to the merest accidents of time and place.

7. On examining our series, therefore, we shall find
it may best be conceived, not necessarily as a succession
nts happening in different ways, but as a succession
ups of things. These groups, on being analysed, are
in every case to be resolvable into collections of sub-

stances and attributes. That which gives its unity succession of groups is the fact of some of these substa attributes being common to the whole succession; that gives their distinction to the groups in the succession fact of some of them containing only a portion of thes stances and attributes, the other portion or portions occasionally absent. So understood, our phraseolog be made to embrace every class of things of which bility can take account.

§ 8. It will be easily seen that the ordinary exp (viz. the 'event,' and the 'way in which it happens') included in the above. When the occasional attribu unimportant the permanent ones are sufficient to f appropriate the name, the presence or absence of the being simply denoted by some modification of the n the addition of some predicate. We may therefore in a cases speak of the collection of attributes as 'the ev the same event essentially, that is—only saying that it to preserve its nominal identity) happens in differen in the different cases. When the occasional attribute ever are important, or compose the majority, this speaking becomes less appropriate; language is som strained by our implying that two extremely different blages are in reality the same event, with a differen in its mode of happening. The phrase is however convenient one, and with this caution against its bein understood, it will frequently be made use of here.

§ 9. A series of the above-mentioned kind is, prehend, the ultimate basis upon which all the r Probability must be based. It is essential to a clea prehension of the subject to have carried our anal to this point, but any attempt at further analysis in intimate nature of the events composing the series,

ired. It is altogether unnecessary, for instance, to form
opinion upon the questions discussed in metaphysics as
he independent existence of substances. We have dis-
ered, on examination, a series composed of groups of
tances and attributes, or of attributes alone. At such
ries we stop, and thence investigate our rules of infer-
e; into what these substances or attributes would them-
es be ultimately analysed, if taken in hand by the
chologist or metaphysician, it is no business of ours to
ire here.

10. The stage then which we have now reached is
of having discovered a quantity of things (they prove
nalysis to be groups of things) which are capable of
g classified together, and are best regarded as consti-
ng a series. The distinctive peculiarity of this series is
finding in it an order, gradually emerging out of disorder,
showing in time a marked and unmistakeable uniformity.
The impression which may possibly be derived from the
ription of such a series, and which the reader will pro-
y already entertain if he have studied Probability before,
at the gradual evolution of this order is indefinite, and
pproach therefore to perfection unlimited. And many of
examples commonly selected certainly tend to confirm
an impression. But in reference to the theory of the
ect it is, I am convinced, an error, and one liable to lead
uch confusion.

The lines which have been prefixed as a motto to this
, "So careful of the type she seems, so careless of the
life," are soon after corrected by the assertion that
ype itself, if we regard it for a long time, changes,
en vanishes and is succeeded by others. So in Pro-
y; that uniformity which is found in the long run,
hich presents so great a contrast to the individual

disorder, though durable is not everlasting. Keep on wa
ing it long enough, and it will be found almost invariabl
fluctuate, and in time may prove as utterly irreducibl
rule, and therefore as incapable of prediction, as the i
vidual cases themselves. The full bearing of this fact u
the theory of the subject, and upon certain common m
of calculation connected with it, will appear more full
some of the following chapters; at present we will co
ourselves to very briefly establishing and illustrating it.

Let us take, for example, the average duration of
This, provided our data are sufficiently extensive, is kno
be tolerably regular and uniform. This fact has been al
indicated in the preceding sections, and is a truth i
of which the popular mind has a tolerably clear grasp a
present day. But a very little consideration will show
there may be a superior as well as an inferior lim
the extent within which this uniformity can be obs
in other words whilst we may fall into error by takin
few instances we may also fail in our aim, though in a
different way and from quite different reasons, by takin
many. At the present time the average duration of l
England may be, say, forty years; but a century ago it
decidedly less; several centuries ago it was presumably
much less; whilst if we possessed statistics referring to
earlier population of the country we should probably fin
there has been since that time a still more marked imp
ment. What may be the future tendency no man can s
certain. It may be, and we hope that it will be the
that owing to sanitary and other improvements, the du
of life will go on increasing steadily; it is at least concei
though doubtless incredible, that it should do so wi
limit. On the other hand, and with much more likel
this duration might gradually tend towards some

Or, again, it is perfectly possible that future gene-
might prefer a short and a merry life, and therefore
their average longevity. The duration of life cannot
pend to some extent upon the general tastes, habits
ployments of the people, that is upon the ideal which
nsciously or unconsciously set before them, and he would
h man who should undertake to predict what this ideal
some centuries hence. All that it is here necessary
r to indicate is, that this particular uniformity (as we
itherto called it, in order to mark its relative cha-
has varied, and, under the influence of future eddies
ion and practice, may vary still; and this to any
and with any degree of irregularity. To borrow a
om Astronomy, we find our uniformity subject to what
be called an irregular *secular* variation.

1. The above is a fair typical instance. If we had
a less simple feature than the length of life, or one
sely connected with what may be called by compari-
great permanent uniformities of nature, we should
und the peculiarity under notice exhibited in a far
triking degree. The deaths from small-pox, for ex-
or the instances of duelling or accusations of witch-
f examined during a few successive decades, might
own a very tolerable degree of uniformity. But these
nities have risen possibly from zero; after various and
reat fluctuations seem tending towards zero again, at
n this century; and may, for anything we know, un-
still more rapid fluctuations in future. Now these
les must be regarded as being only extreme ones, and
h very extreme ones, of what is the almost universal
nature. I shall endeavour to show that even the few
nt exceptions, such as the proportions between male
male births, &c., may not be, and probably in reality

are not, strictly speaking, exceptions. A type, that is, w shall be in the fullest sense of the words, persistent invariable is scarcely to be found in nature. The full im of this conclusion will be seen in future chapters. Atten is only directed here to the important inference that, alth statistics are notoriously of no value unless they are in cient numbers, yet it does not follow but that in certain we may have too many of them. If they are made to tensive, they may again fall short, at least for any parti time or place, of their greatest attainable accuracy.

§ 12. These natural uniformities then are foun length to be subject to fluctuation. Now contrast with any of the uniformities afforded by games of chance; latter seem to show no trace of secular fluctuation, ho long we may continue our examination of them. Criti will be offered, in the course of the following chapters, some of the common attempts to prove *à priori* that must be this fixity in the uniformity in question, but existence there can scarcely be much doubt. Pence heads and tails about equally often now, as they did they were first tossed, and as we believe they will co to do, so long as the present order of things continues. fixity of these uniformities may not be as absolute commonly supposed, but no amount of experience whi need take into account is likely in any appreciable deg interfere with them. Hence the obvious contrast, whereas natural uniformities at length fluctuate, th forded by games of chance seem fixed for ever.

§ 13. Here then are series apparently of two di kinds. They are alike in their initial irregularity, ali their subsequent regularity; it is in what we may their ultimate form that they begin to diverge from other. The one tends without any irregular va

wards a fixed numerical proportion in its uniformity; in
e other the uniformity is found at last to fluctuate, and to
ctuate, it may be, in a manner utterly irreducible to rule.

As this chapter is intended to be little more than ex-
anatory and illustrative of the foundations of the science,
e remark may be made here (for which subsequent justi-
cation will be offered) that it is in the case of series of the
rmer kind only that we are able to make anything which
n be interpreted into strict scientific inferences. We shall
e able however in a general way to see the kind and extent
error that would be committed if, in any example, we were
substitute an imaginary series of the former kind for any
tual series of the latter kind which experience may present
us. The two series are of course to be as alike as possible
all respects, except that the variable uniformity has been
placed by a fixed one. The difference then between them
uld not appear in the initial stage, for in that stage the
tinctive characteristics of the series of Probability are not
parent; all is there irregularity, and it would be as im-
ssible to show that they were alike as that they were
fferent; we can only say generally that each shows the
me kind of irregularity. Nor would it appear in the next
bsequent stage, for the real variability of the uniformity
s not for some time scope to make itself perceived. It
uld only be in what we have called the ultimate stage,
hen we suppose the series to extend for a very long time,
at the difference would begin to make itself felt[1]. The
oportion of persons, for example, who die each year at the
e of six months is, when the numbers examined are on a

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[1] We might express it thus:—a
instances are not sufficient to
play a law at all; a considerable
nber will suffice to display it; but it takes a very great number to establish that a *change* is taking place in the law.

small scale, utterly irregular; it becomes however re; when the numbers examined are on a larger scale; but continued our observation for a very great length of tin over a very great extent of country, we should find regularity itself changing in an irregular way. The stitution just mentioned is really equivalent to saying us assume that the regularity is fixed and permanent. making a hypothesis which may not be altogether consi with fact, but which is forced upon us for the purpo securing precision of statement and definition.

§ 14. The full meaning and bearing of such a su tution will only become apparent in some of the subseq chapters, but it may be pointed out at once that it is in way only that we can with perfect strictness introduce notion of a 'limit' into our account of the matter, at rate in reference to many of the applications of the su to purely statistical enquiries. We say that a certain portion begins to prevail among the events in the long but then on looking closer at the facts we find that we to express ourselves hypothetically, and to say that if pr circumstances remain as they are, the long run will sho characteristics without disturbance. When, as is often case, we know nothing accurately of the circumstance which the succession of events is brought about, but strong reasons to suspect that these circumstances are l to undergo some change, there is really nothing else t done. We can only introduce the conception of a limi wards which the numbers are tending, by assuming these circumstances do not change; in other words, by stituting a series with a fixed uniformity for the actual with the varying uniformity[1].

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[1] The mathematician may illustrate the nature of this substitution by the analogies of the 'circle of curv in geometry, and the 'instants

5. If the reader will study the following example, l known to mathematicians under the name of the urg[1] problem, he will find that it serves to illustrate of the considerations mentioned in this chapter. It especially to bring out the facts that the series with we are concerned must be regarded as indefinitely ve in point of number or duration; and that when so d certain series, but certain series only (the one in n being a case in point), take advantage of the in- range to keep on producing individuals in it whose on from the previous average has no finite limit er. When rightly viewed it is a very simple problem, has given rise, at one time or another, to a good confusion and perplexity.

problem may be stated thus:—a penny is tossed up; ves head I receive one pound; if heads twice running unds; if heads three times running four pounds, and the amount to be received doubling every time that head succeeds. That is, I am to go on as long as it ies to give a succession of heads, to regard this suc- as a 'turn' or set, and then take another turn, and so d for each such turn I am to receive a payment; the nce of tail being understood to yield nothing, in fact mitted from our consideration. However many times ay be given in succession, the number of pounds I aim is found by raising two to a power one less

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astronomy. In the cases these conceptions are made have a phenomenon which uously varying and also its rate of variation. We some given moment, sup- ate at that moment to be then complete its career on that supposition.

[1] So called from its first mathematical treatment appearing in the *Commentarii* of the Petersburg Academy; a variety of notices upon it will be found in Mr Todhunter's History of the Theory of Probability.

than that number of times. Here then is a series formed
a succession of throws. We will assume,—what many pe
sons will consider to admit of demonstration, and wh
certainly experience confirms within considerable limits,
that the rarity of these 'runs' of the same face is in dire
proportion to the amount I receive for them when they
occur. In other words, if we regard only the occasions
which I receive payments, we shall find that every ot
time I get one pound, once in four times I get two pou
once in eight times four pounds, and so on without any
The question is then asked, what ought I to pay for
privilege? At the risk of a slight anticipation of the res
of a subsequent chapter, we may assume that this is equi
lent to asking, what amount paid each time would on
average leave me neither winner nor loser? In other wo
what is the average amount I should receive on the ab
terms? Theory pronounces that I ought to give an *infi*
sum: that is, no finite sum, however great, would be
adequate equivalent. And this is really quite intelligi
There is a series of indefinite length before me, and
longer I continue to work it the richer are my returns,
this without any limit whatever. It is true that the
rich hauls are extremely rare, but still they do come,
when they come they make it up by their greater richn
On every occasion on which people have devoted themse
to the pursuit in question, they made acquaintance, of cou
with but a limited portion of this series; but the series
which we base our calculation is unlimited; and the
ferences usually drawn as to the sum which ought in the
run to be paid for the privilege in question are in per
accordance with this supposition.

The common form of objection is given in the reply,
so far from paying an infinite sum, no sensible man wo

re anything approaching to £50 for such a chance. Pro- bly not, because no man would see enough of the series to ake it worth his while. What most persons form their actical opinion upon, is such small portions of the series they have actually seen or can reasonably expect. Now any such portion, say one which embraces 100 turns, the ngest succession of heads would not amount on the average more than seven or eight. This is observed, but it is for- otten that the formula which produced these, would, if it d greater scope, keep on producing better and better ones ithout any limit. Hence it arises that some persons are erplexed, because the conduct they would adopt, in reference the curtailed portion of the series which they are practically kely to meet with, does not find its justification in inferences hich are necessarily based upon the series in the complete- ess of its infinitude.

§ 16. This will be more clearly seen by considering the rious possibilities, and the scope required in order to exhaust em, when we confine ourselves to a *limited* number of rows. Begin with three. This yields eight equally likely ssibilities. In four of these cases the thrower starts with il and therefore loses: in two he gains a single point e. £1); in one he gains two points, and in one he gains four ints. Hence his total gain being eight pounds achieved in ur different contingencies, his average gain would be two unds.

Now suppose he be allowed to go as far as n throws, so at we have to contemplate 2^n possibilities. All of these ve to be taken into account if we wish to consider what ppens on the average. It will readily be seen that, when the possible cases have been reckoned once, his total gain l be (reckoned in pounds),

$$2^{n-2} + 2^{n-3}.2 + 2^{n-4}.2^2 + \dots\dots + 2.2^{n-3} + 2^{n-2} + 2^{n-1},$$

viz. $(n+1)\,2^{n-2}$.

This being spread over 2^{n-1} different occasions of gain average gain will be $\frac{1}{2}(n+1)$.

Now when we are referring to averages it must b membered that the minimum number of different occurre necessary in order to justify the average is that which en each of them to present itself once. A man proposes to short at a succession of ten heads. Well and good. We him that his average gain will be £5. 10*s*. 0*d*.: but we impress upon him that in order to justify this statemen must commence to toss at least 1024 times, for in no number can all the contingencies of gain and loss be exhi and balanced. If he proposes to reach an average ga £20, he will require to be prepared to go up to 39 th To *justify* this payment he must commence to thro times, i.e. about a million million times. Not before he accomplished this will he be in a position to prove to sceptic that this is the true average value of a 'turn' ext ing to 39 successive tosses.

Of course if he elects to toss to all eternity we adopt the line of explanation which alone is possible w questions of infinity in respect of number and magnitud involved. We cannot tell him to pay down 'an infinite s for this has no strict meaning. But we tell him that, how much he may consent to pay each time runs of heads o he will attain at last a stage in which he will have back his total payments by his total receipts. How large n may be, if he perseveres in trying 2^n times he have a true average receipt of $\frac{1}{2}(n+1)$ pounds, and i continues long enough onwards he *will* have it.

The problem will recur for consideration in a fu chapter.

CHAPTER II.

FURTHER DISCUSSION UPON THE NATURE OF THE SERIES MENTIONED IN THE LAST CHAPTER.

1. IN the course of the last chapter the nature of a par- cular kind of series, that namely, which must be considered o constitute the basis of the science of Probability, has re- ived a sufficiently general explanation for the preliminary urpose of introduction. One might indeed say more than is; for the characteristics which were there pointed out are ally sufficient in themselves to give a fair general idea of he nature of Probability, and of the sort of problems with hich it deals. But in the concluding paragraphs an indi- ation was given that the series of this kind, as they actu- lly occur in nature or as the results of more or less artificial roduction, are seldom or never found to occur in such a imple form as might possibly be expected from what had reviously been said; but that they are almost always seen o be associated together in groups after a somewhat com- licated fashion. A fuller discussion of this topic must now e undertaken.

We will take for examination an instance of a kind with hich the investigations of Quetelet will have served to miliarize some readers. Suppose that we measure the eights of a great many adult men in any town or country. These heights will of course lie between certain extremes in each direction, and if we continue to accumulate our me sures it will be found that they tend to lie continuou between these extremes; that is to say, that under th circumstances no intermediate height will be found to permanently unrepresented in such a collection of measur ments. Now suppose these heights to be marshalled in t order of their magnitude. What we always find is som thing of the following kind;—about the middle point b tween the extremes, a large number of the results will found crowded together: a little on each side of this po there will still be an excess, but not to so great an exte and so on, in some diminishing scale of proportion, until we get towards the extreme results the numbers thin off become relatively exceedingly small.

The point to which attention is here directed is not mere fact that the numbers thus tend to diminish from middle in each direction, but, as will be more fully explai directly, the *law* according to which this progressive dimi tion takes place. The word 'law' is here used in its mat matical sense, to express the formula connecting together two elements in question, namely, the height itself, and relative number that are found of that height. We sh have to enquire whether one of these elements is a functi of the other, and, if so, what function.

§ 2. After what was said in the last chapter, it ne hardly be insisted upon that the interest and significance such investigations as these are almost entirely depende upon the statistics being very extensive. In one or other Quetelet's works on Social Physics[1] will be found a selecti of measurements of almost every element which the physi frame of man can furnish:—his height, his weight, the m cular power of various limbs, the dimensions of almost ev d organ, and so on. Some of the most extensive of ɛpress the heights of 25,000 Federal soldiers from the of the Potomac, and the circumferences of the chests Scotch militia men taken many years ago. Those h to consult a large repertory of such statistics can-eferred to any better sources than to these and other y the same author[1].

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[1] *Essai de Physique Sociale*, 1869. *Anthropométrie*, 1870.

resting and valuable, however, as are Quetelet's sta- investigations (and much of the importance now dly attached to such enquiries is, perhaps, owing his efforts than to those of any other person), I can- feel convinced that there is much in what he has upon the subject which is erroneous and confusing as the foundations of the science of Probability, and the hical questions which it involves. These errors are any means confined to him, but for various reasons ll be better discussed in the form of a criticism of his or implicit expression of them, than in any more in-nt way.

In the first place then, he always, or almost always, that there can be but one and the same law of ar-ent for the results of our observations, measurements, on, in these statistical enquiries. That is, he as-hat whenever we get a group of such magnitudes g about a mean, and growing less frequent as

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ards later statistics on the ect the reader can refer to ts of the Anthropometrical e of the British Association 0, 1881, 1883;—especially These reports seem to resent a great advance on obtained by Quetelet, and ustify the claim of the Secretary (Mr C. Roberts) that their statistics are "unique in range and numbers". They embrace not merely military recruits—like most of the previous tables—but almost every class and age, and both sexes. Moreover they refer not only to stature but to a number of other physical characteristics.

they depart from that mean, we shall find that th minution of frequency takes place according to or variable law, whatever may be the nature of these nitudes, and whatever the process by which they may been obtained.

That such a uniformity as this should prevail an many and various classes of phenomena would probably surprising in any case. But the full significance of s fact as this (if indeed it were a fact) only becomes ap when attention is directed to the profound distinctions nature and origin of the phenomena which are thus sup to be harmonized by being brought under one compreh principle. This will be better appreciated if we take glance at some of the principal classes into which the with which Probability is chiefly concerned may be di These are of a three-fold kind.

§ 4. In the first place there are the various con tions, and runs of luck, afforded by games of chance. pose a handful, consisting of ten coins, were tossed great many times in succession, and the results were lated. What we should obtain would be something following kind. In a certain proportion of cases, and the most numerous of all, we should find that we g heads and five tails; in a somewhat less proportion of we should have, as equally frequent results, four hea tails, and four tails six heads; and so on in a conti diminishing proportion until at length we came dow very small relative number of cases, to nine heads on and nine tails one head; whilst the least frequent possible would be those which gave all heads or all

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[1] As every mathematician knows, the relative numbers of each of these possible throws are given by the successive terms of the expan $(1+1)^{10}$, viz. 1, 10, 45, 120, 2 210, 120, 45, 10, 1.

e the statistical elements under consideration are, as
rds their origin at any rate, optional or brought about
human choice. They would, therefore, be commonly
ribed as being mainly artificial, but their results ulti-
ely altogether a matter of chance.

Again, in the second place, we might take the accurate
urements—i.e. the actual magnitudes themselves,—of
eat many natural objects, belonging to the same genus
ass; such as the cases, already referred to, of the heights,
ther characteristics of the inhabitants of any district.
human volition or intervention of any kind seem to
little or nothing to do with the matter. It is optional
us to *collect* the measures, but the things measured are
outside our control. They would therefore be com-
ly described as being altogether the production of nature,
it would not be supposed that in strictness chance had
hing whatever to do with the matter.

In the third place, the result at which we are aiming
be some fixed magnitude, one and the same in each
ur successive attempts, so that if our measurements
rigidly accurate we should merely obtain the same
lt repeated over and over again. But since all our
hods of attaining our aims are practically subject to
merable imperfections, the results actually obtained
depart more or less, in almost every case, from the
and fixed value which we are trying to secure. They
be sometimes more wide of the mark, sometimes less
the worse attempts being of course the less frequent.
man aims at a target he will seldom or never hit it
sely in the centre, but his good shots will be more[1]

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at is they will be more densely
ted. If a space *the size of the*
ye be examined in each successive circle, the number of shot marks which it contains will be successively less. The *actual* number

numerous than his bad ones. Here again, then, we
a series of magnitudes (i.e. the deflections of the shots
the point aimed at) clustering about a mean, but pro
in a very different way from those of the last two
In this instance the elements would be commonly reg
as only partially the results of human volition, and c
therefore as being only a co-agent in the effects pro
With these must be classed what may be called *esti*
as distinguished from measurements. By the latte
generally understood the results of a certain amou
mechanism or manipulation; by the former we may u
stand those cases in which the magnitude in questi
determined by direct observation or introspection.
interest and importance of this class, so far as sci
principles are concerned, dates mainly from the investig
of Fechner. Its chief field is naturally to be found am
psychological data.

Other classes of things, besides those alluded to
might readily be given. These however are the classes
which the most extensive statistics are obtainable,
which the most practical importance and interest a
tached. The profound distinctions which separate
origin and character are obvious. If they all reall
display precisely the same law of variation it would
most remarkable fact, pointing doubtless to some
seated identity underlying the various ways, appa
so widely distinct, in which they had been brought
The questions now to be discussed are; Is it the
with any considerable degree of rigour, that only on
of distribution does really prevail? and, in so far a
is so, how does it come to pass?

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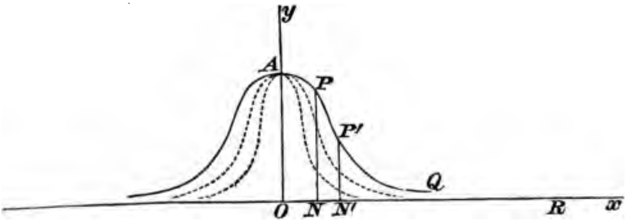
of shots which strike the bull's-eye will not be the greatest, since it covers so much less surface than an other circles.

In support of an affirmative answer to the former two questions, several different kinds of proof are, t be, offered.

For one plan we may make a direct appeal to ce, by collecting sets of statistics and observing their law of distribution. As remarked above, this n done in a great variety of cases, and in some s to a very considerable extent, by Quetelet and His researches have made it abundantly convincing ny classes of things and processes, differing widely nature and origin, do nevertheless appear to conth a considerable degree of accuracy to one and the aw. At least this is made plain for the more

only called the exponential quation being of the form . The curve corresponding s the axis of y at right pressing the fact that near there are a large num- lues approximately equal; ne it begins to slope away wards the axis of x (express- act that the results soon grow less common as we m the mean); and the axis of x is an asymptote in both directions (expressing the fact that no magnitude, however remote from the mean, is strictly impossible; that is, every deviation, however excessive, will have to be encountered at length within the range of a sufficiently long experience). The curve is obviously symmetrical, expressing the fact that equal deviations from the mean, in excess and in defect, tend to occur equally often in the long run.



graphic representation of is given above. For the those unfamiliar with mathematics one or two brief remarks may be here appended concerning some of its properties. (1) It must

central values, for those that is which are situate
nearly about the mean. With regard to the extreme
there is, on the other hand, some difficulty. For i
in the arrangements of the heights of a number c
these extremes are rather a stumbling-block; indeed
been proposed to reject them from both ends of th
on the plea that they are monstrosities, the fact bei
their relative numbers do not seem to be by any
those which theory would assign[1]. Such a plan of re
is however quite unauthorized, for these dwarfs and
are born into the world like their more normall
brethren, and have precisely as much right as any
to be included in the formulæ we draw up.

Besides the instance of the heights of men, other

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not be supposed that all specimens of the curve are similar to one another. The dotted lines are equally specimens of it. In fact, by varying the essentially arbitrary units in which x and y are respectively estimated, we may make the portion towards the vertex of the curve as obtuse or as acute as we please. This consideration is of importance; for it reminds us that, by varying one of these arbitrary units, we could get an 'exponential curve' which should tolerably closely resemble any symmetrical curve of error, provided that this latter recognized and was founded upon the assumption that extreme divergences were excessively rare. Hence it would be difficult, by mere observation, to prove that the law of error in any given case was not exponential; unless the statistics were very extensive, or the actual results departed considerably from th
nential form. (2) It is quit
sible by any graphic repres
to give an adequate idea of t
sive rapidity with which t
after a time approaches the a
At the point R, on our scale, t
would approach within the
thousandth part of an inch
axis of x, a distance which on
good microscope could detect.
as in the hyperbola, e.g. the
approach of the curve to its
tote is continually decreasi
here just the reverse; this
continually increasing. He
two, viz. the curve and the a
appear to the eye, after a ve
time, to merge into one anot

[1] As by Quetelet: noted,
others, by Herschel, *Essa*
409.

bservations of a somewhat similar character have been
dy referred to as collected and arranged by Quetelet.
n the nature of the case, however, there are not many
opriate ones at hand; for when our object is, not to
trate a law which can be otherwise proved, but to
in actual direct proof of it, the collection of observations
measurements ought to be made upon such a large
as to deter any but the most persevering computers
undergoing the requisite labour. Some of the remarks
e in the course of the note on the opposite page will
to illustrate the difficulties which would lie in the way
ch a mode of proof.

We are speaking here, it must be understood, only of
metrical curves: if there is asymmetry, i.e. if the Law of
r is different on different sides of the mean,—a com-
tively very small number of observations would suffice
detect the fact. But, granted symmetry and rapid
ease of frequency on each side of the mean, we could
rally select some one species of the exponential curve
ch should pretty closely represent our statistics in the
hbourhood of the mean. That is, where the statistics are
erous we could secure agreement; and where we could
secure agreement the statistics would be comparatively
carce that we should have to continue the observations
very long time in order to prove the disagreement.

§ 6. Allowing the various statistics such credit as they
rve, for their extent, appropriateness, accuracy and so
the general conclusion which will on the whole be drawn
lmost every one who takes the trouble to consult them,
at they do, in large part, conform approximately to one
or law, at any rate for all except the extreme values.
uch as this must be fully admitted. But that they do
indeed we may say that they cannot, always do so in

the case of the extreme values, will become obvious a little consideration. In some of the classes of thing which the law is supposed to apply, for example, the cessions of heads and tails in the throws of a penny, ther no limit to the magnitude of the fluctuations which may will occur. Postulate as long a succession of heads or of as we please, and if we could only live and toss long eno for it we should succeed in getting it at length. In o cases, including many of the applications of Probab to natural phenomena, there can hardly fail to be limits. Deviations exceeding a certain range may not merely improbable, that is of very rare occurrence, but may often from the nature of the case be actually im sible. And even when they are not actually impossib may frequently appear on examination that they are rendered possible by the occasional introduction of age which are not supposed to be available in the produ of the more ordinary or intermediate values. When, instance, we are making observations with any kind instrument, the nature of its construction may put absolute limit upon the possible amount of error. And if there be not an absolute limit under all kinds of it may nevertheless be the case that there is one fair and proper usage; it being the case that only the instrument is designedly or carelessly tampered with any new causes of divergence be introduced which were confined within the old limits.

Suppose, for instance, that a man is firing at a His worst shots must be supposed to be brought about a combination of such causes as were acting, or prep to act, in every other case; the extreme instance of we may thus term 'fair usage' being when a numb distinct causes have happened to conspire together so end in the same direction, instead of, as in the other s, more or less neutralizing one another's work. But aggregate effect of such causes may well be supposed e limited. The man will not discharge his shot nearly ight angles to the true line of fire unless some entirely r cause comes in, as by some unusual circumstance ring distracted his attention, or by his having had some smodic seizure. But influences of this kind were not pposed to have been available before; and even if they ere we are taking a bold step in assuming that these casional great disturbances are subject to the same kind f laws as are the aggregates of innumerable little ones.

We cannot indeed lay much stress upon an example f this last kind, as compared with those in which we an see for certain that there is a fixed limit to the range f error. It is therefore offered rather for illustration than r proof. The enormous, in fact inconceivable magnitude f the numbers expressive of the chance of very rare com-inations, such as those in question, has such a bewildering ffect upon the mind that one may be sometimes apt to con-und the impossible with the higher degrees of the merely athematically improbable.

§ 7. At the time the first edition of this essay was com-osed writers on Statistics were, I think, still for the most rt under the influence of Quetelet, and inclined to over-alue his authority on this particular subject: of late however ttention has been repeatedly drawn to the necessity of king account of other laws of arrangement than the binomial r exponential.

Mr Galton, for instance,—to whom every branch of the eory of statistics owes so much,—has insisted[1] that the assumption which lies at the basis of the well-known law of

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[1] *Proc. R. Soc.* Oct. 21, 1879.

'Frequency of Error'...is incorrect in many groups of vit and social phenomena....For example, suppose we endeavo to match a tint; Fechner's law, in its approximative an simplest form of sensation = log. stimulus, tells us that series of tints, in which the quantities of white scattered on black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to th eye to be separated by equal intervals of tint. Therefore, matching a grey that contains 8 portions of white, we just as likely to err by selecting one that has 16 portions one that has 4 portions. In the first case there would be error in excess, of 8; in the second there would be an err in deficiency, of 4. Therefore, an error of the same magnitu in excess or in deficiency is not equally probable." The co sequences of this assumption are worked out in a remarkab paper by Dr D. McAlister, to which allusion will have to made again hereafter. All that concerns us here to point o is that when the results of statistics of this character a arranged graphically we do *not* get a curve which is sym metrical on both sides of a central axis.

§ 8. More recently, Mr F. Y. Edgeworth (in a report a Committee of the British Association appointed to enqui into the variation of the monetary standard) has urged th same considerations in respect of prices of commodities. H gives a number of statistics "drawn from the prices of twel commodities during the two periods 1782—1820, 1820—18 The maximum and minimum entry for each series havi been noted, it is found that the number of entries above th 'middle point,' half-way between the maximum and minimu is in every instance less than half the total number of entri in the series. In the twenty-four trials there is not a sing exception to the rule, and in very few cases even an appro

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[1] We are here considering, remember, the case of a *finite* amount of statistics; so that there are act limits at each end.

an exception. We may presume then that the curves are the lop-sided character indicated by the accompanying gram." The same facts are also ascertained in respect to ce variations as distinguished from time variations. To se may be added some statistics of my own, referring to heights of the barometer taken at the same hour on more n 4000 successive days (v. *Nature*, Sept. 2, 1887). So far these go they show a marked asymmetry of arrangement.

In fact it appears to me that this want of symmetry ht to be looked for in all cases in which the phenomena der measurement are of a 'one-sided' character; in the se that they are measured on one side only of a certain d point from which their possibility is supposed to start. not only is it impossible for them to fall below this point: g before they reach it the influence of its proximity is felt enhancing the difficulty and importance of the same ount of absolute difference.

Look at a table of statures, for instance, with a mean lue of 69 inches. A diminution of three feet (were this sible) is much more influential,—counts for much more, every sense of the term,—than an addition of the same ount; for the former does not double the mean, while the ter more than halves it. Revert to an illustration. If ast number of petty influencing circumstances of the kind ady described were to act upon a swinging *pendulum* we uld expect the deflections in each direction to display mmetry; but if they were to act upon a *spring* we should expect such a result. Any phenomena of which the ter is the more appropriate illustration can hardly be ected to range themselves with symmetry about a mean[1].

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[1] It must be admitted that ex- nce has not yet (I believe) shown this asymmetry in respect of heights.

§ 9 (II.). The last remarks will suggest another k proof which might be offered to establish the invariable n of the law of error. It is of a direct deductive kin appealing immediately to statistics, but involving an en into the actual or assumed nature of the causes by whic events are brought about. Imagine that the event consideration is brought to pass, in the first place, by fixed cause, or group of fixed causes. If this compris the influencing circumstances the event would inva happen in precisely the same way: there would be no or deflections whatever to be taken account of. But suppose that there were also an enormous number of small causes which tended to produce deflections; that causes acted in entire independence of one another; and each of the lot told as often, in the long run, in one dir as in the opposite. It is easy[1] to see, in a general way, would follow from these assumptions. In a very few nearly all the causes would tell in the same directio other words, in a very few cases the deflection woul extreme. In a greater number of cases, however, it only be the most part of them that would tell in one tion, whilst a few did what they could to counteract the the result being a comparatively larger number of some smaller deflections. So on, in increasing numbers, ti approach the middle point. Here we shall have a very number of very small deflections: the cases in whic opposed influences just succeed in balancing one an so that no error whatever is produced, being, though ac infrequent, relatively the most frequent of all.

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[1] The above reasoning will probably be accepted as valid at this stage of enquiry. But in strictness, assumptions are made here, which however justifiable they may be in themselves, involve somewhat of a cipation. They demand, an future chapter will receive, scrutiny and criticism.

if all deflections from a mean were brought about in
just indicated (an indication which must suffice for
ent) we should always have one and the same law of
nent of frequency for these deflections or errors, viz.
nential[1] law mentioned in § 5.

. It may be readily admitted from what we know
e production of events that something resembling

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nite numerical example of
of concentration of fre-
out the mean was given in
o § 4. It was of a binomial
sisting of the successive
the expansion of $(1+1)^m$.
y be shown (Quetelet, *Let-*
8; Liagre, *Calcul des Proba-*
l) that the expansion of such
l, as m becomes indefinitely
proaches as its limit the
al form; that is, if we take
r of equidistant ordinates
nal respectively to 1, m,
&c., and connect their ver-
figure we obtain approxi-
presents some form of the
Ae^{-hx^2}, and tends to become
with it, as m is increased
imit. In other words, if
e the errors to be produced
ed number of finite, equal
endent causes, we have an
tion to the exponential
Error, which merges into
s the causes are increased
and diminished in magni-
ut limit. Jevons has given
s of Science, p. 381) a dia-
n to scale, to show how
approximation is. One point must be carefully remembered here, as it is frequently overlooked (by Quetelet, for instance). The coefficients of a binomial of two equal terms—as $(1+1)^m$, in the preceding paragraph—are symmetrical in their arrangement from the first, and very speedily become indistinguishable in (graphical) outline from the final exponential form. But if, on the other hand, we were to consider the successive terms of such a binomial as $(1+4)^m$ (which are proportional to the relative chances of 0, 1, 2, 3,... failures in m ventures, of an event which has one chance in its favour to four against it) we should have an unsymmetrical succession. If however we suppose m to increase without limit, as in the former supposition, the unsymmetry gradually disappears and we tend towards precisely the same exponential form as if we had begun with two equal terms. The only difference is that the position of the vertex of the curve is no longer in the centre: in other words, the likeliest term or event is not an equal number of successes and failures but successes and failures in the ratio of 1 to 4.

these assumptions, and therefore something resembling consequences which follow from them, is really secured i very great number of cases. But although this may pre approximately, it is in the highest degree improbable tha could ever be secured, even artificially, with anything proaching to rigid accuracy. For one thing, the causes deflection will seldom or never be really independent of another. Some of them will generally be of a kind such t the supposition that several are swaying in one directi may affect the capacity of each to produce that full ef which it would have been capable of if it had been left to its work alone. In the common example, for instance, firing at a mark, so long as we consider the case of the tol ably good shots the effect of the wind (one of the causes error) will be approximately the same whatever may be precise direction of the bullet. But when a shot is consid ably wide of the mark the wind can no longer be regarded acting at right angles to the line of flight, and its effect consequence will not be precisely the same as before. other words, the causes here are not strictly independent, they were assumed to be; and consequently the results to attributed to each are not absolutely uninfluenced by th of the others. Doubtless the effect is trifling here, bu apprehend that if we were carefully to scrutinize the mo in which the several elements of the total cause consp together, we should find that the assumption of absol independence was hazardous, not to say unwarrantable, i very great number of cases. These brief remarks upon process by which the deflections are brought about m suffice for the present purpose, as the subject will receiv fuller investigation in the course of the next chapter.

According, therefore, to the best consideration wh can at the present stage be afforded to this subject, we raw a similar conclusion from this deductive line of argu-ment as from the direct appeal to statistics. The same eneral result seems to be established; namely, that approxi-mately, with sufficient accuracy for all practical purposes, we may say that an examination of the causes by which the deflections are generally brought about shows that they are mostly of such a character as would result in giving us the commonly accepted 'Law of Error,' as it is termed[1]. The two lines of enquiry, therefore, within the limits assigned, afford each other a decided mutual confirmation.

§ 11 (III.). There still remains a third, indirect and mathematical line of proof, which might be offered to establish the conclusion that the Law of Error is always one and the same. It may be maintained that the recognized and universal employment of one and the same method, that known to mathematicians and astronomers as the Method of Least Squares, in all manner of different cases with very satisfactory results, is compatible only with the supposition that the errors to which that method is applied must be grouped according to one invariable law. If all 'laws of error' were not of one and the same type, that is, if the relative frequency of large and small divergences (such as we have been speaking of) were not arranged according to one pattern, how could one method or rule equally suit them all?

In order to preserve a continuity of treatment, some notice must be taken of this enquiry here, though, as in the case of the last argument, any thorough discussion of the subject is impossible at the present stage. For one thing, would involve too much employment of mathematics, or any rate of mathematical conceptions, to be suitable for th general plan of this treatise: I have accordingly devoted special chapter to the consideration of it.

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[1] 'Law of Error' is the usual technical term for what has been elsewhere spoken of above as a Law of Divergence from a mean. It is in strictness only appropriate in the case of one, namely the third, of the three classes of phenomena mentioned in § 4, but by a convenient generalization it is equally applied to the other two; so that we term the amount of the divergence from the mean an 'error' in every case, however it may have been brought about.

The main reason, however, against discussing this argu ment here, is, that to do so would involve the anticipation a totally different side of the science of Probability from th hitherto treated of. This must be especially insisted upon, the neglect of it involves much confusion and some erro During these earlier chapters we have been entirely occupi with laying what may be called the physical foundations Probability. We have done nothing else than establish, in o way or another, the existence of certain groups or arrang ments of things which are found to present themselves nature; we have endeavoured to explain how they come pass, and we have illustrated their principal characteristi But these are merely the foundations of Inference, we ha not yet said a word upon the logical processes which are be erected upon these foundations. We have not therefo entered yet upon the *logic* of chance.

§ 12. Now the way in which the Method of Least Squa is sometimes spoken of tends to conceal the magnitude this distinction. Writers have regarded it as synonymo with the Law of Error, whereas the fact is that the two a not only totally distinct things but that they have scarce even any necessary connection with each other. The Law Error is the statement of a physical fact; it simply assign with more or less of accuracy, the relative frequency wit which errors or deviations of any kind are found in practi to present themselves. It belongs therefore to what may b termed the physical foundations of the science. The Metho of Least Squares, on the other hand, is not a law at all in th

tific sense of the term. It is simply a rule or direction
ming us how we may best proceed to treat any group of
errors which may be set before us, so as to extract the
result at which they have been aiming. Clearly there-
it belongs to the inferential or logical part of the subject.
It cannot indeed be denied that the methods we employ
t have some connection with the arrangement of the facts
hich they are applied; but the two things are none the
distinct in their nature, and in this case the connection
not seem at all a necessary one, but at most one of pro-
ty and convenience. The Method of Least Squares is
lly applied, no doubt, to the most familiar and common
of the Law of Error, namely the exponential form with
h we have been recently occupied. But other forms of
of error may exist, and, if they did, the method in
tion might equally well be applied to them. I am not
ting that it would necessarily be the best method in
y case, but it would be a possible one; indeed we may
further and say, as will be shown in a future chapter,
t it would be a good method in almost every case. But
particular merits or demerits do not interfere with its
ible employment in every case in which we may choose
sort to it. It will be seen therefore, even from the few
rks that can be made upon the subject here, that the
that one and the same method is very commonly em-
ed with satisfactory results affords little or no proof that
errors to which it is applied must be arranged according
e fixed law.

13. So much then for the attempt to prove the preva-
e, in all cases, of this particular law of divergence. The
point in Quetelet's treatment of the subject which deserves
tion as erroneous or confusing, is the doctrine maintained
m and others as to the existence of what he terms a type

in the groups of things in question. This is a not unn
consequence from some of the data and conclusions
last few paragraphs. Refer back to two of the three
of things already mentioned in § 4. If it really were th
that in arranging in order a series of incorrect observa
or attempts of our own, and a collection of natural ob
belonging to some one and the same species or class, we
that the law of their divergence was in each case identi
the long run, we should be naturally disposed to app
same expression 'Law of Error' to both instances
though in strictness it could only be appropriate t
former. When we perform an operation ourselves w
clear consciousness of what we are aiming at, we may
correctly speak of every deviation from this as bei
error; but when Nature presents us with a group of
of any kind, it is using a rather bold metaphor to sp
this case also of a law of error, as if she had been aim
something all the time, and had like the rest of us
her mark more or less in almost every instance[1].

Suppose we make a long succession of attempts to me
accurately the precise height of a man, we should fro
cause or another seldom or never succeed in doing so
absolute accuracy. But we have no right to assume that
imperfect measurements of ours would be found so to d
according to one particular law of error as to presen
precise counterpart of a series of actual heights of *di*
men, supposing that these latter were assigned with ab
precision. What might be the actual law of error in a
of direct measurements of any given magnitude could
be asserted beforehand, and probably the attempt to

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[1] This however seems to be the purport, either by direct assertion or by implication, of two elaborate works by Quetelet, v
Physique Sociale, and his *A*
métrie.

e it by experience has not been made sufficiently often to
le us to ascertain it; but upon general grounds it seems
no means certain that it would follow the so-called ex-
ential law. Be this however as it may, it is rather a
ace of language to talk as if nature had been at work in
same way as one of us; aiming (ineffectually for the most
) at a given result, that is at producing a man endowed
a certain stature, proportions, and so on, who might
efore be regarded as the typical man.

§ 14. Stated as above, namely, that there is a fixed
riable human type to which all individual specimens of
anity may be regarded as having been meant to attain,
from which they have deviated in one direction or
ther, according to a law of deviation capable of *à priori*
rmination, the doctrine is little else than absurd. But
e look somewhat closer at the facts of the case, and the
able explanation of these facts, we may see our way to
mportant truth. The facts, on the authority of Quetelet's
istics (the great interest and value of which must be
kly admitted), are very briefly as follows: if we take any
nent of our physical frame which admits of accurate
surement, say the height, and determine this measure in
reat number of different individuals belonging to any
rably homogeneous class of people, we shall find that
e heights do admit of an orderly arrangement about a
n, after the fashion which has been already repeatedly
tioned. What is meant by a homogeneous class? is a
inent and significant enquiry, but applying this condition
ny simple cases its meaning is readily stated. It implies
the mean in question will be different according to the
onality of the persons under measurement. According to
telet[1], in the case of Englishmen the mean is about

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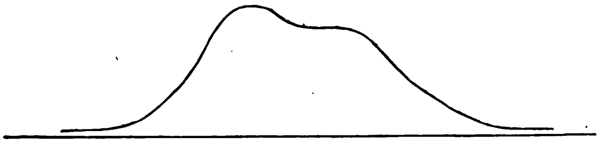
[1] le scarcely, however, professes to give these as an accurate measure

5 ft. 9 in.; for Belgians about 5 ft. 7 in.; for the Frenc 5 ft. 4 in. It need hardly be added that these measu those of adult males.

§ 15. It may fairly be asked here what woul been the consequence, had we, instead of keeping the and the French apart, mixed the results of our measu of them all together? The question is an important o will oblige us to understand more clearly what we n homogeneous classes. The answer that would usu given to it, though substantially correct, is somewhat cisive and summary. It would be said that we a mixing distinctly heterogeneous elements, and that sequence the resultant law of error will be by no n the simple character previously exhibited. So far as answer is to be admitted its grounds are easy to app In accordance with the usual law of error the dive from the mean grow continuously less numerous increase in amount. Now, if we mix up the Fren English heights, what will follow? Beginning fr English mean of 5 feet 9 inches, the heights will at f low almost entirely the law determined by these conditions, for at this point the English data are very rous, and the French by comparison very few. But begin to approach the French mean, the numbers wi to show that continual diminution which they shoul according to the English scale of arrangement, for h French data are in turn very numerous, and the En comparison few. The result of such a combination of

of the mean height, nor does he always give precisely the same measure. Practically, none but soldiers being measured in any great numbers, the English stature did not afford accurate data on scale. The statistics giv pages further on are pro more trustworthy.

eous elements is illustrated by the figure annexed, of rse in a very exaggerated form.



§ 16. In the above case the nature of the heterogeneity, the reasons why the statistics should be so collected and anged as to avoid it, seemed tolerably obvious. It will be n still more plainly if we take a parallel case drawn from ficial proceedings. Suppose that after a man had fired a thousand shots at a certain spot, say a wafer fixed someere on a wall, the position of the spot at which he aims e shifted, and he fired a few thousand more shots at the fer in its new position. Now let us collect and arrange all e shots of both series in the order of their departure from her of the centres, say the new one. Here we should lly be mingling together two discordant sets of elements, her of which, if kept apart from the other, would have en of a simple and homogeneous character. We should d, in consequence, that the resultant law of error betrayed composite or heterogeneous origin by a glaring departure m the customary form, somewhat after the fashion indited in the above diagram.

The instance of the English and French heights resemes the one just given, but falls far short of it in the strinncy with which the requisite conditions are secured. The t is we have not here got the most suitable requirements, a group consisting of a few fixed causes supplemented by umerable little disturbing influences. What we call a tion is really a highly artificial body, the members of which are subject to a considerable number of local o casional disturbing causes. Amongst Frenchmen wer cluded, presumably, Bretons, Provençals, Alsatians, and thus commingling distinctions which, though less than between French and English, regarded as wholes, are far from being insignificant. And to these differenc race must be added other disturbances, also highly i tant, dependent upon varying climate, food and occup It is plain, therefore, that whatever objections exist ag confusing together French and English statistics, exist though of course in a less degree, against confusing tog those of the various provincial and other components w make up the French people.

§ 17. Out of the great variety of important c which influence the height of men, it is probable that which most nearly fulfil the main conditions required by 'Law of Error' are those about which we know the Upon the effects of food and employment, observation something to say, but upon the purely physiological ca by which the height of the parents influences the heigh the offspring, we have probably nothing which deserve be called knowledge. Perhaps the best supposition we make is one which, in accordance with the saying that ' breeds like', would assume that the purely physiolog causes represent the constant element; that is, given a ho geneous race of people to begin with, who freely in marry, and are subject to like circumstances of climate, f and occupation, the standard would remain on the wh constant[1].

In such a case the man who possessed the mean hei mean weight, mean strength, and so on, might then

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[1] This statement will receive some explanation and correction in the chapter.

, in a sort of way, a 'type'. The deviations from this
would then be produced by innumerable small influ-
partly physiological, partly physical and social, acting
e most part independently of one another, and result-
a Law of Error of the usual description. Under such
tions and explanations as these, there seems to be no
able objection to speaking of a French or English type
an. But it must always be remembered that under
resent circumstances of every political nation, these
hat heterogeneous bodies might be subdivided into
s smaller groups, each of which would frequently ex-
the characteristics of such a type in an even more
d degree.

18. On this point the reports of the Anthropometrical
ittee, already referred to, are most instructive. They
ate the extent to which this subdivision could be
l out, and prove,—if any proof were necessary,—that
scovery of Quetelet's *homme moyen* would lead us a
hase. So far as their results go the mean 'English'
(in inches) is 67·66. But this is composed of Scotch,
English and Welsh constituents, the separate means of
being, respectively; 68·71, 67·90, 67·36, and 66·66.
ese again may be subdivided; for careful observation
that the mean English stature is distinctly greater in
districts (e.g. the North-Eastern counties) than in
Then again the mean of the professional classes is
erably greater than that of the labourers; and that of
nest and intelligent is very much greater than that of
iminal and lunatic constituents of the population.
o far as the observations are extensive enough for the
e, it appears that every characteristic in respect of the
ng about a mean which can be detected in the more
ive of these classes can be detected also in the nar-

rower. Nor is there any reason to suppose that the process of subdivision could not be carried out as farther as we chose to prolong it.

§ 19. It need hardly be added to the above re that no one who gives the slightest adhesion to the Do of Evolution could regard the type, in the above qu sense of the term, as possessing any real permanence fixity. If the constant causes, whatever they may b main unchanged, and if the variable ones continue i long run to balance one another, the results will conti cluster about the same mean. But if the constant undergo a gradual change, or if the variable ones, inste balancing each other suffer one or more of their num begin to acquire a preponderating influence, so as to sort of bias upon their aggregate effect, the mean w once begin, so to say, to shift its ground. And having begun to shift, it may continue to do so, to whatever we recognize that Species are variable and Developme fact. It is as if the point on the target at which we ai stead of being fixed, were slowly changing its position continue to fire at it; changing almost certainly to som tent and temporarily, and not improbably to a conside extent and permanently.

§ 20. Our examples throughout this chapter have almost exclusively drawn from physical characte whether of man or of inanimate things; but it need n supposed that we are necessarily confined to such inst Mr Galton, for instance, has proposed to extend the principles of calculation to mental phenomena, with a to their more accurate determination. The objects gained by so doing belong rather to the inferential p our subject, and will be better indicated further o they do not involve any distinct principle. Like oth mpts to apply the methods of science in the region of the nd, this proposal has met with some opposition; with very ght reason, as it seems to me. That our mental qualities, they could be submitted to accurate measurement, would found to follow the usual Law of Error, may be assumed hout much hesitation. The known extent of the correla- n of mental and bodily characteristics gives high proba- lity to the supposition that what is proved to prevail, at y rate approximately, amongst most bodily elements which ve been submitted to measurement, will prevail also ongst the mental elements.

To what extent such measurements could be carried t practically, is another matter. It does not seem to that it could be done with much success; partly be- se our mental qualities are so closely connected with, deed so run into one another, that it is impossible to late them for purposes of comparison[1]. This is to some tent indeed a difficulty in bodily measurements, but it is r more so in those of the mind, where we can hardly get yond what can be called a good guess. The doctrine, erefore, that mental qualities follow the now familiar law arrangement can scarcely be grounded upon anything ore than a strong analogy. Still this analogy is quite ong enough to justify us in accepting the doctrine and the conclusions which follow from it, in so far as our imates and measurements can be regarded as trustworthy. ere seems therefore nothing unreasonable in the attempt establish a system of natural classification of mankind arranging them into a certain number of groups above d below the average, each group being intended to correspond to certain limits of excellency or deficiency that is necessary for such a purpose is that the : departure from the mean should be tolerably constant widely different circumstances: in this case through the races of man. Of course if the law of diverg the same as that which prevails in inanimate nat have a still wider and more natural system of classi at hand, and one which ought to be familiar, more or every one who has thus to estimate qualities.

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[1] I am not speaking here of the familiar results of Psychophysics, ch are mainly occupied with the measurement of perceptions and other simple states of consciousness.

§ 21. Perhaps one of the best illustrations of th timate application of such principles is to be found Galton's work on *Hereditary Genius.* Indeed the ful and purport of some of his reasonings there can har appreciated except by those who are familiar with th ceptions which we have been discussing in this chapte can only afford space to notice one or two points, b student will find in the perusal, of at any rate th argumentive parts, of that volume[2] an interesting illus of the doctrines now under discussion. For one th may be safely asserted, that no one unfamiliar with th of Error would ever in the least appreciate the ex

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[1] Perhaps the best brief account of Mr Galton's method is to be found in a paper in *Mind* (July, 1880) on the statistics of Mental Imagery. The subject under comparison here —viz. the relative power, possessed by different persons, of raising clear visual images of objects no longer present to us—is one which it seems impossible to 'measure', in the ordinary sense of the term. But by arranging all the answers in the order in which the faculty in question seems to be possessed we can, with some approach to s select the middlemost perso row and use him as a basis parison with the correspon son in any other batch. A larly with those who occu relative positions than tha middlemost.

[2] I refer to the introduc concluding chapters: the bu book is, from the nature of mainly occupied with statis biographical details.

pidity with which the superior degrees of excellence tend become scarce. Every one, of course, can see at once, in numerical way at least, what is involved in being 'one of a illion'; but they would not at all understand, how very tle extra superiority is to be looked for in the man who is e of two million'. They would confound the mere nu- rical distinction, which seems in some way to imply uble excellence, with the intrinsic superiority, which ould mostly be represented by a very small fractional ad- ntage. To be 'one of ten million' sounds very grand, but the qualities under consideration could be estimated in emselves without the knowledge of the vastly wider area m which the selection had been made, and in freedom erefore from any consequent numerical bias, people would surprised to find what a very slight comparative superi- ty was, as a rule, thus obtained.

§ 22. The point just mentioned is an important one in guments from statistics. If, for instance, we find a small oup of persons, connected together by blood-relationship, d all possessing some mental characteristic in marked periority, much depends upon the comparative rarity of ch excellence when we are endeavouring to decide whether not the common possession of these qualities was acci- ntal. Such a decision can never be more than a rough e, but if it is to be made at all this consideration must ter as a factor. Again, when we are comparing one nation th another[1], say the Athenian with any modern European ople, does the popular mind at all appreciate what sort of idence of general superiority is implied by the production, of one nation, of such a group as can be composed of rates, Plato, and a few of their contemporaries? In this

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See Galton's *Hereditary Genius*, pp. 336—350, "On the comparative th of different races."

latter case we are also, it should be remarked, employin 'Law of Error' in a second way; for we are assuming where the extremes are great so will also the means other words we are assuming that every amount of depa from the mean occurs with a (roughly) calculable de of relative frequency. However generally this truth be accepted in a vague way, its evidence can only be preciated by those who know the reasons which can be g in its favour.

But the same principles will also supply a cautio the case of the last example. They remind us that the mere purpose of comparison, the *average* man of group or class is a much better object for selection the eminent one. There may be greater difficulties in way of detecting him, but when we have done so we got possession of a securer and more stable basis of parison. He is selected, by the nature of the case, the most numerous stratum of his society; the emi man from a thinly occupied stratum. In accordance th fore with the now familiar laws of averages and of numbers the fluctuations amongst the former will gene be very few and small in comparison with those amongst latter.

CHAPTER III.

ON THE CAUSAL PROCESS BY WHICH THE GROUPS OR SERIES OF PROBABILITY ARE BROUGHT ABOUT.

1. In discussing the question whether all the various oups and series with which Probability is concerned are of ecisely one and the same type, we made some examination the process by which they are naturally produced, but we ust now enter a little more into the details of this pro- ss. All events are the results of numerous and com- icated antecedents, far too numerous and complicated fact for it to be possible for us to determine or take em all into account. Now, though it is strictly true that can never determine them all, there is a broad dis- ction between the case of Induction, in which we can ke out enough of them, and with sufficient accuracy, to tisfy a reasonable certainty, and Probability, in which we nnot do so. To Induction we shall return in a future hapter, and therefore no more need be said about it here.

We shall find it convenient to begin with a division hich, though not pretending to any philosophical accuracy, ill serve as a preliminary guide. It is the simple division to objects, and the agencies which affect them. All the enomena with which Probability is concerned (as indeed ost of those with which science of any kind is concerned) e the product of certain objects natural and artificial, ting under the influence of *certain* agencies natural and artificial. In the tossing of a penny, for instance, the obje would be the penny or pence which were successiv thrown; the agencies would be the act of throwing, a everything which combined directly or indirectly with t to make any particular face come uppermost. This i simple and intelligible division, and can easily be so tended in meaning as to embrace every class of objects w which we are concerned.

Now if, in any two or more cases, we had the sa object, or objects indistinguishably alike, and if they w exposed to the influence of agencies in all respects preci alike, we should expect the results to be precisely sim By one of the applications of the familiar principle of uniformity of nature we should be confident that ex likeness in the antecedents would be followed by ex likeness in the consequents. If the same penny, or sin pence, were thrown in exactly the same way, we sh invariably find that the same face falls uppermost.

§ 2. What we actually find is, of course, very far moved from this. In the case of the objects, when t are artificial constructions, e.g. dice, pence, cards, it is that they are purposely made as nearly as possible in tinguishably alike. We either use the same thing over over again or different ones made according to prec the same model. But in natural objects nothing of sort prevails. In fact when we come to examine them find reproduced in them precisely the same characteri as those which present themselves in the final result w we were asked to explain, so that unless we examine t a stage further back, as we shall have to do to some ex at any rate, we seem to be merely postulating again very peculiarity of the phenomena which we were un taking to explain. They will be found, for instanc

nsist of large classes of objects, throughout all the indi-
dual members of which a general resemblance extends.
ppose that we were considering the length of life. The
jects here are the human beings, or that selected class
them, whose lives we are considering. The resemblance
isting among them is to be found in the strength and
undness of their principal vital organs, together with all
e circumstances which collectively make up what we call
e goodness of their constitutions. It is true that most of
se circumstances do not admit of any approach to actual
asurement; but, as was pointed out in the last chapter,
y many of the circumstances which do admit of such
asurement have been measured, and found to display
e characteristics in question. Hence, from the known
alogy and correlation between our various organs, there
n be no reasonable doubt that if we could arrange human
nstitutions in general, or the various elements which com-
se them in particular, in the order of their strength, we
ould find just such an aggregate regularity and just such
oupings about the mean, as the final result (viz. in this
se the length of their lives) presents to our notice.

§ 3. It will be observed therefore that for this pur-
se the existence of natural kinds or groups is necessary.
our games of chance of course the same die may be
rown, or a card be drawn from the same pack, as often
we please; but many of the events which occur to
man beings either cannot be repeated at all, or not often
ough to secure in the case of the single individual any
ficient statistical uniformity. Such regularity as we trace
nature is owing, much more than is often suspected,
the arrangement of things in natural kinds, each of
em containing a large number of individuals. Were each
d of animals or vegetables *limited* to a single pair, or

even to but a few pairs, there would not be much left for the collection of statistical tables amongst Or to take a less violent supposition, if the nu in each natural class of objects were much smaller they are at present, or the differences between their ties and sub-species much more marked, the conse difficulty of extracting from them any sufficient leng statistical tables, though not fatal, might be very se A large number of objects in the class, together with general similarity which entitles the objects to be comprised in one class, seem to be important condi for the applicability of the theory of Probability t phenomenon. Something analogous to this excessive p of objects in a class would be found in the attem apply special Insurance offices to the case of those where the numbers are very limited, and the emplo so dangerous as to put them in a class by themselve an insurance society were started for the workm gunpowder mills alone, a premium would have to be ch to avoid possible ruin, so high as to illustrate the ex paucity of appropriate statistics.

§ 4. So much (at present) for the objects. If we to what we have termed the agencies, we find muc same thing again here. By the adjustment of their re intensity, and the respective frequency of their occur the total effects which they produce are found to b tolerably uniform. It is of course conceivable that should have been otherwise. It might have been that the second group of conditions so exactly correcte former as to convert the merely general uniformity an absolute one; or it might have been found, o other hand, that the second group should aggrava disturb the influence of the former to such an to destroy all the uniformity of its effects. Practically ither is the case. The second condition simply varies the tails, leaving the uniformity on the whole of precisely e same general description as it was before. Or if the bjects were supposed to be absolutely alike, as in the case successive throws of a penny, it may serve to bring about uniformity. Analysis will show these agencies to be us made up of an almost infinite number of different mponents, but it will detect the same peculiarity that have so often had occasion to refer to, pervading almost these components. The proportions in which they are mbined will be found to be nearly, though not quite, the me; the intensity with which they act will be nearly ough not quite equal. And they will all unite and blend to a more and more perfect regularity as we proceed to ke the average of a larger number of instances.

Take, for instance, the length of life. As we have seen, e constitutions of a very large number of persons selected random will be found to present much the same feature; neral uniformity accompanied by individual irregularity. ow when these persons go out into the world, they are posed to a variety of agencies, the collective influence which will assign to each the length of life allotted to m. These agencies are of course innumerable, and their utual interaction complicated beyond all power of analysis extricate. Each effect becomes in its turn a cause, is terwoven inextricably with an indefinite number of other uses, and reacts upon the final result. Climate, food, thing, are some of these agencies, or rather comprise gregate groups of them. The nature of a man's work also important. One man overworks himself, another lows an unhealthy trade, a third exposes himself to in-tion, and so on.

The result of all this interaction between what we h
thus called objects and agencies is that the final outc
presents the same general characteristics of uniformity as
be detected separately in the two constituent elements.
rather, as we shall proceed presently to show, it does
in the great majority of cases.

§ 5. It may be objected that such an explanation
the above does not really amount to anything deser
of the name, for that instead of explaining how a partic
state of things is caused it merely points out that
same state exists elsewhere. There is a uniformity
covered in the objects at the stage when they are c
monly submitted to calculation; we then grope al
amongst the causes of them, and after all only disc
a precisely similar uniformity existing amongst these ca
This is to some extent true, for though part of the objec
can be removed, it must always remain the case that
foundations of an objective science will rest in the last re
upon the mere fact that things are found to be of such
such a character.

§ 6. This division, into objects and the agencies w
affect them, is merely intended for a rough practical
rangement, sufficient to point out to the reader the im
diate nature of the causes which bring about our fam
uniformities. If we go back a step further, it might fa
be maintained that they may be reduced to one, nam
to the agencies. The objects, as we have termed th
are not an original creation in the state in which we
find them. No one supposes that whole groups or cla
were brought into existence simultaneously, with all t
general resemblances and particular differences fully
veloped. Even if it were the case that the first pai
of each natural kind had been specially created, ins

being developed out of pre-existing forms, it would still rue that amongst the numbers of each that now present mselves the characteristic differences and resemblances the result of what we have termed agencies. Take, for ance, a single characteristic only, say the height; what ermines this as we find it in any given group of men? tly, no doubt, the nature of their own food, clothing, ployment, and so on, especially in the earliest years of r life; partly also, very likely, similar conditions and umstances on the part of their parents at one time or ther. No one, I presume, in the present state of know- ge, would attempt to enumerate the remaining causes, even to give any indication of their exact nature; but the same time few would entertain any doubt that ncies of this general description have been the determin- causes at work.

If it be asked again, Into what may these agencies mselves be ultimately analysed? the answer to this estion, in so far as it involves any detailed examination them, would be foreign to the plan of this essay. In so as any general remarks, applicable to nearly all classes e of such agencies, are called for, we are led back to point from which we started in the previous chapter, en we were discussing whether there is necessarily one d law according to which all our series are formed. We re saw that every event might be regarded as being ught about by a comparatively few important causes, of kind which comprises all of which ordinary observation es any notice, and an indefinitely numerous group of ll causes, too numerous, minute, and uncertain in their on for us to be able to estimate them or indeed to take m individually into account at all. The important ones, s true, may also in turn be themselves conceived to be made up of aggregates of small components, but the still best regarded as being by comparison simple and tinct, for their component parts act mostly in groups lectively, appearing and disappearing together, so that possess the essential characteristics of unity.

§ 7. Now, broadly speaking, it appears to me tha most suitable conditions for Probability are these: tha important causes should be by comparison fixed and manent, and that the remaining ones should on the ave continue to act as often in one direction as in the o This they may do in two ways. In the first plac may be able to predicate nothing more of them than mere fact that they act[1] as often in one direction as other; what we should then obtain would be merely simple statistical uniformity that is described in the chapter. But it may be the case, and in practice gene is so more or less approximately, that these minor c act also in independence of one another. What we get is a group of uniformities such as was explained illustrated in the second chapter. Every possible co nation of these causes then occurring with a regular gree of frequency, we find one peculiar kind of unifo exhibited, not merely in the mere fact of excess and d (of whatever may be the variable quality in question), also in every particular amount of excess and d Hence, in this case, we get what some writers te 'mean' or 'type,' instead of a simple average. Fo stance, suppose a man throwing a quoit at a mark. our fixed causes are his strength, the weight of the q

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[1] As stated above, this is really little more than a re-statement, a stage further back, of the existence of the same kind of uniformity as that which we are called upon plain in the concrete detail sented to us in experience.

: intention of aiming at a given point. These we ' course suppose to remain unchanged, if we are in any such uniformity as we are seeking. The ,nd variable causes are all those innumerable little ng influences referred to in the last chapter. It :onceivably be the case that we were only able to n that these acted as often in one direction as in er; what we should then find was that the quoit to fall short of the mark as often as beyond it. ing to these little causes being mostly independent another, and more or less equal in their influence, also that every *amount* of excess and defect presents e general characteristics, and that in a large number vs the quantity of divergences from the mark, of any mount, is a tolerably determinate function, according ular law, of that amount of divergence[1].

The necessity of the conditions just hinted at ;t be seen by a reference to cases in which any of

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rould seem in fact that in ıd rude observations the ceed from a *very few* prin- es, and in consequence our s [as to the Exponential :rror] will probably repre- 'acts only imperfectly, and ncy of the errors will only ıte roughly and vaguely to hich follows from it. But onomers, not content with e of accuracy they had rosecuted their researches emaining sources of error, d that not three or four, *t number* of minor sources nearly co-ordinate import- ın to reveal themselves, having been till then masked and overshadowed by the graver errors which had been now approximately removed......There were errors of graduation, and many others in the contraction of instruments; other errors of their adjustments; errors (technically so called) of *observation;* errors from the changes of temperature, of weather, from slight irregular motions and vibrations; in short, the thousand minute disturbing influences with which modern astronomers are familiar." (Extracted from a paper by Mr Crofton in the Vol. of the *Philosophical Transactions* for 1870, p. 177.)

them happen to be missing. Thus we know that the l of life is on the whole tolerably regular, and so are numbers of those who die in successive years or cent of most of the commoner diseases. But it does not to be the case with all diseases. What, for instanc the Sweating Sickness, the Black Death, the A Cholera? The two former either do not recur, or, if do, recur in such a mild form as not to deserve the name. What in fact of any of the diseases which epidemic rather than endemic? All these have their ca doubtless, and would be produced again by the recurr of the conditions which caused them before. But som them apparently do not recur at all. They seem to depended upon such rare conditions that their occurr was almost unique. And of those which do recur the co is frequently so eccentric and irregular, often so much pendent upon human will or want of will, as to enti deprive their results (that is, the annual number of de which they cause) of the statistical uniformity of which are speaking.

The explanation probably is that one of the princ causes in such cases is what we commonly call contag If so, we have at once a cause which so far from being fix subject to the utmost variability. Stringent caution destroy it, carelessness may aggravate it to any extent. will of man, as finding its expression either on the part government, of doctors, or of the public, may make pretty nearly what is wished, though against the possib of its entrance into any community no precautions can solutely insure us.

§ 9. If it be replied that this want of statistical re larity only arises from the fact of our having confined selves to too limited a time, and that we should gularity disappear here, as elsewhere, if we kept our les open long enough, we shall find that the answer will gest another case in which the requisite conditions for bability are wanting. Such a reply would only be con- ive upon the supposition that the ways and thoughts of n are in the long run invariable, or if variable, subject to iodic changes only. On the assumption of a steady pro- ss in society, either for the better or the worse, the argu- nt falls to the ground at once. From what we know of course of the world, these fearful pests of the past may considered as solitary events in our history, or at least nts which will not be repeated. No continued uniformity ld therefore be found in the deaths which they occasion, gh the registrar's books were kept open for a thousand rs. The reason here is probably to be sought in the dual alteration of those indefinitely numerous conditions ich we term collectively progress or civilization. Every le circumstance of this kind has some bearing upon the bility of any one to catch a disease. But when a kind of w and steady tide sets in, in consequence of which these uences no longer remain at about the same average ength, warring on about equal terms with hostile in- ences, but on the contrary show a steady tendency to in- ase their power, the statistics will, with consequent steadi- s and permanence, take the impress of such a change.

§ 10. Briefly then, if we were asked where the dis- ctive characteristics of Probability are most prominently be found, and where they are most prominently absent, might say that (1) they prevail principally in the pro- rties of natural kinds, both in the ultimate and in the de- tive or accidental properties. In all the characteristics of ural species, in all they do and in all which happens to m, so far as it depends upon their properties, we seldom fail to detect this regularity. Thus in men; their hei strength, weight, the age to which they live, the diseas which they die; all present a well-known uniformity. I insurance tables offer the most familiar instance of the i portance of these applications of Probability.

(2) The same peculiarity prevails again in the fo and frequency of most natural agencies. Wind and weat are seen to lose their proverbial irregularity when exami on a large scale. Man's work therefore, when operated by such agencies as these, even though it had been made different cases absolutely alike to begin with, afterw shows only a general regularity. I may sow exactly same amount of seed in my field every year. The yield one year be moderate, the next year be abundant thro favourable weather, and then again in turn be destroyed hail. But in the long run these irregularities will be equali in the result of my crops, because they are equalized in power and frequency of the productive agencies. T business of underwriters, and offices which insure the cr against hail, would fall under this class; though, as alre remarked, there is no very profound distinction between th and the former class.

The reader must be reminded again that this fixity only temporary, that is, that even here the series belong the class of those which possess a fluctuating type. Th indeed who believe in the fixity of natural species will h the best chance of finding a series of the really perman type amongst them, though even they will admit that so change in the characteristic is attainable in length of ti In the case of the principal natural agencies, it is of co incontestable that the present average is referable to present geological period only. Our average temperat and average rainfall have in former times been wid

erent from what they now are, and doubtless will be so
in.

Any fuller investigation of the process by which, on the
eory of Evolution, out of a primeval simplicity and uni-
mity the present variety was educed, hardly belongs to
scope of the present work: at most, a few hints must
ice.

§ 11. The above, then, are instances of natural objects
natural agencies. There seems reason to believe that it
n such things only, as distinguished from things artificial,
t the property in question is to be found. This is an as-
tion that will need some discussion and explanation. Two
ances, in apparent opposition, will at once occur to the
d of some readers; one of which, from its great intrinsic
ortance, and the other, from the frequency of the pro-
ms which it furnishes, will demand a few minutes' separate
mination.

(1) The first of these is the already mentioned case of
trumental observations. In the use of astronomical and
er instruments the utmost possible degree of accuracy is
en desired, a degree which cannot be reasonably hoped for
any one single observation. What we do therefore in
se cases is to make a large number of successive observa-
ns which are naturally found to differ somewhat from each
er in their results; by means of these the true value
explained in a future chapter, on the Method of Least
uares) is to be determined as accurately as possible. The
jects then of calculation here are a certain number of
ments, slightly incorrect elements, given by successive
ervations. Are not these observations artificial, or the
ect product of voluntary agency? Certainly not: or rather,
answer depends on what we understand by voluntary.
t is really intended and aimed at by the observer, is of

course, perfect accuracy, that is, the true observation, or
voluntary steps and preliminaries on which this observa
depends. Whether voluntary or not, this result only car
called intentional. But this result is not obtained. W
we actually get in its place is a series of deviations fron
containing results more or less wide of the truth. Now
what are these deviations caused? By just such agencie
we have been considering in some of the earlier section
this chapter. Heat and its irregular warping influe
draughts of air producing their corresponding effects, (
and consequent friction in one part or another, the sli
distortion of the instrument by strains or the slow une
contraction which continues long after the metal was c
these and such as these are some of the causes which di
us from the truth. Besides this group, there are otl
which certainly do depend upon human agency, but wl
are not, strictly speaking, voluntary. They are such as
irregular action of the muscles, inability to make our var
organs and members execute precisely the purposes we h
in mind, perhaps different rates in the rapidity of the n
vous currents, or in the response to stimuli, in the sam
different observers. The effect produced by some of th
and the allowance that has in consequence to be made,
becoming familiar even to the outside world under the n
of the 'personal equation' in astronomical, psychophys
and other observations.

§ 12. (2) The other example, alluded to above, is the s
one of cards and dice. Here, as in the last case, the re
is remotely voluntary, in the sense that deliberate voli
presents itself at one stage. But subsequently to this st
the result is produced or affected by so many involun
agencies that it owes its characteristic properties to th
The turning up, for example, of a particular face of a d

e result of voluntary agency, but it is not an immediate sult. That particular face was not chosen, though the fact f its being chosen was the remote consequence of an act of hoice. There has been an intermediate chaos of conflicting gencies, which no one can calculate before or distinguish fterwards. These agencies seem to show a uniformity in he long run, and thence to produce a similar uniformity in he result. The drawing of a card from a pack is indeed ore directly volitional, as in cutting for partners in a game f whist. But no one continues to do this long without aving the pack well shuffled in the interval, whereby a host f involuntary influences are let in.

§ 13. The once startling but now familiar uniformities xhibited in the cases of suicides and misdirected letters, do ot belong to the same class. The final resolution, or want f it, which leads to these results, is in each case indeed an mportant ingredient in the individual's action or omission; ut, in so far as volition has anything to do with the results s *a whole*, it instantly disturbs them. If the voice of the egislature speaks out, or any great preacher or moralist ucceeds in deterring, or any impressive example in in-uencing, our moral statistics are instantly tampered with. ome further discussion will be devoted to this subject in a uture chapter; it need only be remarked here that (always xcluding such common or general influence as those just entioned) the average volition, potent as it is in each eparate case, is on the whole swayed by non-voluntary con-itions, such as those of health, the casualties of employ-ent, &c., in fact the various circumstances which influence he length of a man's life.

§ 14. Such distinctions as those just insisted on may em to some persons to be needless, but serious errors have ccasionally arisen from the neglect of them. The immediate products of man's mind, so far indeed as we can an attempt to obtain them, do not seem to possess essential characteristic of Probability. Their characte seems rather to be, either perfect mathematical accura utter want of it, either law unfailing or mere caprice. e.g., we find the trees in a forest growing in straight we unhesitatingly conclude that they were planted by as they stand. It is true on the other hand, that if we them not regularly planted, we cannot conclude that were not planted by man; partly because the planter have worked without a plan, partly because the subseq irregularities brought on by nature may have obscured plan. Practically the mind has to work by the aid of perfect instruments, and is subjected to many hindra through various and conflicting agencies, and by these m the work loses its original properties. Suppose, for inst that a man, instead of producing numerical results by perfect observations or by the cast of dice, were to s them at first hand for himself by simply thinking of t at once; what sort of series would he obtain? It woul about as difficult to obtain in this way any such serie those appropriate to Probability as it would be to keep heart or pulse working regularly by direct acts of voli supposing that he had the requisite control over these org But the mere suggestion is absurd. A man must hav object in thinking, he must think according to a rule or mula; but unless he takes some natural series as a cop will never be able to construct one mentally which shall manently imitate the originals. Or take another produ human efforts, in which the intention can be executed tolerable success. When any one builds a house, there many slight disturbing influences at work, such as shrin of bricks and mortar, settling of foundations, &c. But effect which these disturbances are able to produce is so inappreciably small, that we may fairly consider that the result obtained is the direct product of the mind, the accurate realization of its intention. What is the consequence? Every house in the row, if designed by one man and at one time, is of exactly the same height, width, &c. as its neighbours; or if there are variations they are few, definite, and regular. The result offers no resemblance whatever to the heights, weights, &c. of a number of men selected at random. The builder probably had some regular design in contemplation, and he has succeeded in executing it.

§ 15. It may be replied that if we extend our observations, say to the houses of a large city, we shall then detect the property under discussion. The different heights of a great number, when grouped together, might be found to resemble those of a great number of human beings under similar treatment. Something of this kind might not improbably be found to be the case, though the resemblance would be far from being a close one. But to raise this question is to get on to different ground, for we were speaking (as remarked above) not of the work of different minds with their different aims, but of that of one mind. In a multiplicity of designs, there may be that variable uniformity, for which we may look in vain in a single design. The heights which the *different* builders contemplated might be found to group themselves into something of the same kind of uniformity as that which prevails in most other things which they should undertake to do independently. We might then trace the action of the same two conditions,—a uniformity in the multitude of their different designs, a uniformity also in the infinite variety of the influences which have modified those designs. But this is a very different thing from saying that the work of one man will show such a result as this. The difference is much lik that between the tread of a thousand men who are steppin without thinking of each other, and their tread when the are drilled into a regiment. In the former case there i the working, in one way or another, of a thousand mind in the latter, of one only.

The investigations of this and the former chapt constitute a sufficiently close examination into the detail causes by which the peculiar form of statistical results wit which we are concerned is actually produced, to serve th purpose of a work which is occupied mainly with the *metho* of the Science of Probability. The great importance, how ever, of certain statistical or sociological enquiries will d mand a recurrence in a future chapter to one particul application of these statistics, viz. to those concerned wit some classes of human actions.

§ 16. The only important addition to, or modification o the foregoing remarks which I have found occasion to ma is due to Mr Galton. He has recently pointed out,—and w I believe the first to do so,—that in certain cases som analysis of the causal processes can be effected, and is in fa absolutely necessary in order to account for the facts ob served. Take, for instance, the *heights* of the population o any country. If the distribution or dispersion of these abo their mean value were left to the unimpeded action of tho myriad productive agencies alluded to above, we should ce tainly obtain such an arrangement in the posterity of an one generation as had already been exhibited in the parent That is, we should find repeated in the previous stage th same kind of order as we were trying to account for in th following stage.

But then, as Mr Galton insists, if such agencies acte freely and independently, though we should get the sam

d of arrangement or distribution, we should not get the
e *degree* of it: there would, on the contrary, be a tendency
wards further dispersion. The 'curve of facility' (v. the
gram on p. 29) would belong to the same class, but would
ve a different modulus. We shall see this at once if we
e for comparison a case in which similar agencies work
ir way without any counteraction whatever. Suppose, for
ance, that a large number of persons, whose fortunes
e equal to begin with, were to commence gambling or
ing continually for some small sum. If we examine
r circumstances after successive intervals of time, we
uld expect to find their fortunes distributed according to
same general law,—i.e. the now familiar law in ques-
n,—but we should also expect to find that the poorest
s were slightly poorer, and the richest ones slightly
er, on each successive occasion. We shall see more
ut this in a future chapter (on *Gambling*), but it may
taken for granted here that there is nothing in the laws
chance to resist this tendency towards intensifying the
tremes.

Now it is found, on the contrary, in the case of vital
enomena,—for instance in that of height, and presumably
most of the other qualities which are in any way character-
c of natural kinds,—that there is, through a number of
cessive generations, a remarkable degree of fixity. The
men are not taller, and the short men are not shorter,
cent. of the population in successive generations: always
posing of course that some general change of circum-
nces, such as climate, diet, &c. has not set in. There
t therefore here be some cause at work which tends, so
ay, to draw in the extremes and thus to check the other-
continually increasing dispersion.

§ 17. The facts were first tested by careful experiment.

At the date of Mr Galton's original paper on the subj there were no available statistics of heights of human bei so a physical element admitting of careful experiment (the size or weight of certain seeds) was accurately estima From these data the actual amount of reversion from extremes, that is, of the slight pressure continually put u the extreme members with the result of crowding them towards the mean, was determined, and this was comp with what theory would require in order to keep the cha teristics of the species permanently fixed. Since t statistics have been obtained to a large extent which directly with the heights of human beings.

The general conclusion at which we arrive is that t are several causes at work which are neither slight no dependent. There is, for instance, the observed fact that extremes are as a rule not equally fertile with the me nor equally capable of resisting death and disease. H as regards their mere numbers, there is a tendency for t somewhat to thin out. Then again there is a dist positive cause in respect of 'reversion.' Not only are offspring of the extremes less numerous, but these offsp also tend to cluster about a mean which is, so to say, shi a little towards the true centre of the whole group; i.e wards the mean offspring of the mean parents.

§ 18. For a full discussion of these characteristics, for a variety of most ingenious illustrations of their mo agency and of their comparative efficacy, the reader ma referred to Mr Galton's original articles. For our pre purpose it will suffice to say that these characteristics towards maintaining the fixity of species; and that th they do not affect what may be called the general natu

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[1] *Typical Laws of Heredity;* read before the Royal Institution, Feb. 9, 1877. See also *Journal of th throp. Inst.* Nov. 1885.

e 'probability curve' or 'law of facility', they do determine precise value in the cases in question. If, indeed, it be ed why there is no need for any such corrective influence the case of, say, firing at a mark: the answer is that there no opening for it except where a *cumulative* influence is induced. The reason why the fortunes of our betting party owed an ever increasing divergency, and why some special rection was needed in order to avert such a tendency in e case of vital phenomena, was that the new starting-point every step was slightly determined by the results of the evious step. The man who has lost a shilling one time rts, next time, worse off by just a shilling; and, but for e corrections we have been indicating, the man who was n tall would, so to say, throw off his descendants from a ntage ground of superior height. The true parallel in the e of the marksmen would be to suppose that their new ints of aim were always shifted a little in the direction of e last divergence. The spreading out of the shot-marks ould then continue without limit, just as would the diver- nce of fortunes of the supposed gamblers.

CHAPTER IV.

ON THE MODES OF ESTABLISHING AND DETERMININ EXISTENCE AND NUMERICAL PROPORTIONS OF THE RACTERISTIC PROPERTIES OF OUR SERIES OR GR

§ 1. At the point which we have now reached, w
supposed to be in possession of series or groups of a c
kind, lying at the bottom, as one may say, and formin
foundation on which the Science of Probability is
erected. We have described with sufficient particulari
characteristics of such a series, and have indicated th
cess by which it is, as a rule, actually brought abo
nature. The next enquiries which have to be succes
made are, how in any particular case we are to est
their existence and determine their special characte
properties? and secondly[1], when we have obtained th
what mode are they to be employed for logical purpose

The answer to the former enquiry does not seem dif
Experience is our sole guide. If we want to discover w
in reality a series of *things*, not a series of our own co
tions, we must appeal to the things themselves to obt
for we cannot find much help elsewhere. We cann
how many persons will be born or die in a year, o
many houses will be burnt or ships wrecked, without ac
counting them. When we thus speak of 'experienc

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[1] This latter enquiry belongs to what may be termed the more purely logical part of this volume, entered on in the course of Cha

to employ the term in its widest signification; we experience supplemented by all the aids which in-ɜ or deductive logic can afford. When, for instance, ʌe found the series which comprises the numbers of s of any assigned class who die in successive years, we ιo hesitation in extending it some way into the future as into the past. The justification of such a proce-nust be sought in the ordinary canons of Induction. special discussion will be given upon the connection n Probability and Induction, no more need be said his subject here; but nothing will be found there at ce with the assertion just made, that the series we y are ultimately obtained by experience only.

In many cases it is undoubtedly true that we do not to direct experience at all. If I want to know what is ance of holding ten trumps in a game of whist, I do quire how often such a thing has occurred before. If inhabitants of the globe were to divide themselves up hist parties they would have to keep on at it for a many years, if they wanted to settle the question ctorily in that way. What we do of course is to cal-algebraically the proportion of possible combinations ich ten trumps can occur, and take this as the answer problem. So again, if I wanted to know the chance owing six with a die whose faces were unequal, it be a question if my best way would not be to calculate trically the solid angle subtended at the centre of y by the opposite face, and the ratio of this to the surface of a sphere would represent sufficiently closely ance required.

is quite true that in such examples as the above, espe-the former one, nobody would ever think of appealing istics. This would be a tedious process to adopt when, as here, the mechanical and other conditions upon whic production of the events depend are comparatively fe terminate, and admit of isolated consideration, whilst enormous number of combinations which can be constr out of them causes an enormous consequent multiplici ways in which the events can possibly happen. Henc practice, *à priori* determination is often easy, whilst *à* *riori* appeal to experience would be not merely tediou utterly impracticable. This, combined with the freq simplicity and attractiveness of such examples when de ively treated, has made them very popular, and produce impression in many quarters that they are the proper ty instances to illustrate the theory of chance. Whereas the science been concerned with those kinds of events which in practice are commonly made subjects of insur probably no other view would ever have been taken that it was based upon direct appeal to experience.

§ 3. When, however, we look a little closer, we find there is no occasion for such a sharp distinction as tha parently implied between the two classes of examples indicated. In such cases as those of dice and cards, eve which we appear to reason directly from the determi conditions, or possible variety of the events, rather than actual observation of their occurrence, we shall find that procedure is only valid by the help of a tacit assump which can never be determined otherwise than by d experience. It is, no doubt, an exceedingly natural obvious assumption, and one which is continually deri fresh weight from every-day observation, but it is one w ought not to be admitted without consideration. As this very important matter, not so much in itself as in conne with the light which it throws upon the theory of the su we will enter into a somewhat detailed examination of

t us take a very simple example, that of tossing up a
. Suppose that I am contemplating a succession of
ırows; I can see that the only possible events are[1]
HT., *TH.*, *TT.* So much is certain. We are more-
tolerably well convinced from experience that these
ı occur, in the long run, about equally often. This is
rse admitted on all hands. But on the view commonly
ained, it is contended that we might have known the
əforehand on grounds which are applicable to an indefi-
.umber of other and more complex cases. The form in
this view would generally be advanced is, that we are
d to state beforehand that the four throws above men-
ı are *equally likely*. If in return we ask what is meant
e expression 'equally likely', it appears that there are
ıd only two possible forms of reply. One of these seeks
planation in the state of mind of the observer, the
seeks it in some characteristic of the things observed.

) It might, for instance, be said on the one hand, that
is meant is that the four events contemplated are
y easy to imagine, or, more accurately, that our ex-
ion or belief in their occurrence is equal. We could
r be content with this reply, for the further enquiry
immediately be urged, On what ground is this to be
ed? What are the characteristics of events of which
pectation is equal? If we consented to give an answer
s further enquiry, we should be led to the second form
ly, to be noticed directly; if we did not consent we
l, it seems, be admitting that Probability was only a

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the use of those not ac-
d with the common notation
d in this subject, it may be
d that *HH* is simply an
ated way of saying that the two successive throws of the penny give head; *HT* that the first of them gives head, and the second tail; and so on with the remaining symbols.

portion of Psychology, confined therefore to considering of mind in themselves, rather than in their reference to viz. as being true or false. We should, that is, be c to make it a science of inference about things. This will have to be gone into more thoroughly in another ter; but it is impossible to direct attention too promin to the fact that Logic (and therefore Probability as a b of Logic) is not concerned with what men *do* believ with what they ought to believe, if they are to b correctly.

(2) In the other form of reply the explanation o phrase in question would be sought, not in a state of but in a quality of the things contemplated. We might the following as the meaning, viz. that the events would occur with equal frequency in the long run. ground of this assertion would probably be found in pa perience, and it would doubtless be impossible so to the answer as to exclude the notion of our belief altog But still there is a broad distinction between seekin equality in the amount of our belief, as before, and i frequency of occurrence of the events themselves, as her

§ 4. When we have got as far as this it can readi shown that an appeal to experience cannot be long ev For *can* the assertion in question (viz. that the throws o penny will occur equally often) be safely made *à pr* Those who consider that it can seem hardly to have faced the difficulties which meet them. For when we b to enquire seriously whether the penny will really do is expected of it, we find that restrictions have to b troduced. In the first place it must be an ideal coin, its sides equal and fair. This restriction is perfectly in gible; the study of solid geometry enables us to idea penny into a circular or cylindrical lamina. But this con

itself is not sufficient, others are wanted as well. The
ny was supposed to be tossed up, as we say 'at random.'
at is meant by this, and how is this process to be ideal-
d? To ask this is to introduce no idle subtlety; for it
ld scarcely be maintained that the heads and tails would
their fair chances if, immediately before the throwing,
were so to place the coin in our hands as to start it
ys with the same side upwards. The difference that
ld result in consequence, slight as its cause is, would
d in time to show itself in the results. Or, if we per-
ed in starting with each of the two sides alternately
ards, would the longer repetitions of the same side get
r fair chance?

Perhaps it will be replied that if we think nothing what-
r about these matters all will come right of its own accord.
ay, and doubtless will be so, but this is falling back upon
erience. It is here, then, that we find ourselves resting on
experimental assumption above mentioned, and which
eed cannot be avoided. For suppose, lastly, that the
umstances of nature, or my bodily or mental constitution,
e such that the same side always *is* started upwards, or
eed that they are started in any arbitrary order of our
n? Well, it will be replied, it would not then be a fair
l. If we press in this way for an answer to such enquiries,
shall find that these tacit restrictions are really nothing
than a mode of securing an experimental result. They
only another way of saying, Let a series of actions be
formed in such a way as to secure a sequence of a par-
lar kind, viz., of the kind described in the previous
pters.

§ 5. An intermediate way of evading the direct appeal
xperience is sometimes found by defining the probability
n event as being measured by the ratio which the

number of cases favourable to the event bears to the number of cases which are possible. This seems a somew loose and ambiguous way of speaking. It is clearly enough to *count* the number of cases merely, they must be *valued*, since it is not certain that each is equally pot in producing the effect. This, of course, would never denied, but sufficient importance does not seem to be tached to the fact that we have really no other way valuing them except by estimating the effects which t actually do, or would produce. Instead of thus appealin the proportion of cases favourable to the event, it is far be (at least as regards the foundation of the science, for we not at this moment discussing the practical method of fa tating our calculations) to appeal at once to the proport of cases in which the event actually occurs.

§ 6. The remarks above made will apply, of course most of the other common examples of chance; the throw of dice, drawing of cards, of balls from bags, &c. In last case, for instance, one would naturally be inclined suppose that a ball which had just been put back wo thereby have a better chance of coming out again next ti since it will be more in the way for that purpose. How this to be prevented? If we designedly thrust it to middle or bottom of the others, we may overdo the prec tion; and are in any case introducing human design, t element so essentially hostile to all that we understand chance. If we were to trust to a good shake setting matt right, we may easily be deceived; for shaking the bag hardly do more than diminish the disposition of those b which were already in each other's neighbourhood, to rem so. In the consequent interaction of each upon all, arrangement in which they start cannot but leave its im to some extent upon their final positions. In all such c herefore, if we scrutinize our language, we shall find that ny supposed *à priori* mode of stating a problem is little else han a compendious way of saying, Let means be taken for btaining a given result. Since it is upon this result that ur inferences ultimately rest, it seems simpler and more hilosophical to appeal to it at once as the groundwork of ur science.

§ 7. Let us again take the instance of the tossing of a enny, and examine it somewhat more minutely, to see what n be actually proved about the results we shall obtain. e are willing to give the pence fair treatment by assuming at they are perfect, that is, that in the long run they show o preference for either head or tail; the question then mains, Will the repetitions of the same face obtain the oportional shares to which they are entitled by the ual interpretations of the theory? Putting then, as before, r the sake of brevity, H for head, and HH for heads twice nning, we are brought to this issue;—Given that the ance of H is ½, does it follow necessarily that the chance of H (with two pence) is ¼? To say nothing of 'H ten times' curring once in 1024 times (with ten pence), need it occur all? The mathematicians, for the most part, seem to think at this conclusion follows necessarily from first principles; me it seems to rest upon no more certain evidence than a sonable extension by Induction.

Taking then the possible results which can be obtained om a pair of pence, what do we find? Four different ults may follow, namely, (1) HT, (2) HH, (3) TH, (4) TT. it can be proved that these four are equally probable, that occur equally often, the commonly accepted conclusions ll follow, for a precisely similar argument would apply to the larger numbers.

§ 8. The proof usually advanced makes use of what is called the Principle of Sufficient Reason. It take form;—Here are four kinds of throws which may h once admit that the separate elements of them, nam and T, happen equally often, and it will follow that the combinations will also happen equally often, for no reas be given in favour of one of them that would not equall in favour of the others.

To a certain extent we must admit the validity principle for the purpose. In the case of the throws above, it would be valid to prove the equal frequency and (3) and also of (2) and (4); for there is no diff existing between these pairs except what is introduced own notation[1]. TH is the same as HT, except in the of the occurrence of the symbols H and T, which we take into account. But either of the pair (1) and different from either of the pair (2) and (4). Transpo notation, and there would still remain here a distinction the mind can recognize. A succession of the same twice running is distinguished from the conjunction different things, by a distinction which does not d upon our arbitrary notation only, and would remain e unaltered by a change in this notation. The principle fore of Sufficient Reason, if admitted, would only prov doublets of the two kinds, for example (2) and (4), equally often, but it would not prove that they must ar once in four times. It cannot be proved indeed in this y that they need ever occur at all.

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[1] I am endeavouring to treat this rule of Sufficient Reason in a way that shall be legitimate in the opinion of those who accept it, but there seem very great doubts whether a contradiction is not involved when we attempt to extract results from it. If the sides are absolutely alike, how can there be any difference between the terms of the series succession seems then reduc dull uniformity, a mere itera the same thing many time series we contemplated has peared. If the sides are no lutely alike, what becomes applicability of the rule?

§ 9. The formula, then, not being demonstrable *à priori*, might have been concluded,) can it be obtained by exience? To a certain extent it can; the present experience ankind in pence and dice seems to show that the smaller cessions of throws do really occur in about the proportions igned by the theory. But how nearly they do so no one say, for the amount of time and trouble to be expended ore we could feel that we have verified the fact, even for ll numbers, is very great, whilst for large numbers it ld be simply intolerable. The experiment of throwing en enough to obtain 'heads ten times' has been actually formed by two or three persons, and the results are given De Morgan, and Jevons[1]. This, however, being only ficient on the average to give 'heads ten times' a single nce, the evidence is very slight; it would take a conerable number of such experiments to set the matter arly at rest.

Any such rule, then, as that which we have just been cussing, which professes to describe what will take place a long succession of throws, is only conclusively proved by perience within very narrow limits, that is, for small repeions of the same face; within limits less narrow, indeed, feel assured that the rule cannot be flagrantly in error, erwise the variation would be almost sure to be detected. om this we feel strongly inclined to infer that the same will hold throughout. In other words, we are inclined extend the rule by Induction and Analogy. Still there so many instances in nature of proposed laws which hold in narrow limits but get egregiously astray when we attempt to push them to great lengths, that we must giv best but a qualified assent to the truth of the formula.

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[1] *Formal Logic*, p. 185. *Principles of Science*, p. 208.

§ 10. The object of the above reasoning is simpl show that we cannot be certain that the rule is true. us now turn for a minute to consider the causes by w the succession of heads and tails is produced, and we perhaps see reasons to make us still more doubtful.

It has been already pointed out that in calculating pr bilities *à priori*, as it is called, we are only able to do so introducing restrictions and suppositions which are in rea equivalent to assuming the expected results. We use w which in strictness mean, Let a given process be perform but an analysis of our language, and an examination various tacit suppositions which make themselves felt moment they are not complied with, soon show that our meaning is, Let a series of a given kind be obtained; it i this series only, and not to the conditions of its product that all our subsequent calculations properly apply. physical process being performed, we want to know whet anything resembling the contemplated series really will obtained.

Now if the penny were invariably set the same side permost, and thrown with the same velocity of rotation to the same height, &c.—in a word, subjected to the s conditions,—it would always come down with the same uppermost. Practically, we know that nothing of this k occurs, for the individual variations in the results of throws are endless. Still there will be an *average* of th conditions, about which the throws will be found, as it w to cluster much more thickly than elsewhere. We should inclined therefore to infer that if the same side were alw set uppermost there would really be a departure from sort of series which we ordinarily expect. In a very l

mber of throws we should probably begin to find, under
ch circumstances, that either head or tail was having a
eference shown to it. If so, would not similar effects be
nd to be connected with the way in which we started
ch successive *pair* of throws? According as we chose to
ke a practice of putting HH or TT uppermost, might
ere not be a disturbance in the proportion of successions of
o heads or two tails? Following out this train of reason-
g, it would seem to point with some likelihood to the con-
sion that in order to obtain a series of the kind we
pect, we should have to dispose the antecedents in a
milar series at the start. The changes and chances pro-
ced by the act of throwing might introduce infinite in-
vidual variations, and yet there might be found, in the very
g run, to be a close similarity between these two series.

§ 11. This is, to a certain extent, only shifting the dif-
lty, I admit; for the claim formerly advanced about the
sibility of proving the proportions of the throws in the
mer series, will probably now be repeated in favour of
ose in the latter. Still the question is very much nar-
wed, for we have reduced it to a series of *voluntary* acts.
man may put whatever side he pleases uppermost. He
y act consciously, as I have said, or he may think nothing
atever about the matter, that is, throw at random; if so,
will probably be asserted by many that he will involun-
rily produce a series of the kind in question. It may be
, or it may not; it does not seem that there are any
ily accessible data by which to decide. All that I am
cerned with here is to show the likelihood that the com-
nly received result does in reality depend upon the ful-
ment of a certain condition at the outset, a condition which
is certainly optional with any one to fulfil or not as he
ases. The short successions doubtless will take care of

themselves, owing to the infinite complications produce the casual variations in throwing; but the long ones suffer, unless their interest be consciously or unconscio regarded at the outset.

§ 12. The advice, 'Only try long enough, and you sooner or later get any result that is possible,' is plaus but it rests only on Induction and Analogy; mathematic not prove it. As has been repeatedly stated, there are distinct views of the subject. Either we may, on the hand, take a series of symbols, call them heads and t H, T, &c.; and make the assumption that each of these, each pair of them, and so on, will occur in the long run a regulated degree of frequency. We may then calcu their various combinations, and the consequences that be drawn from the data assumed. This is a purely algebr process; it is infallible; and there is no limit whatever to extent to which it may be carried. This way of lookin the matter may be, and undoubtedly should be, not more than the counterpart of what I have called the sul tuted or idealized series which generally has to be introd as the basis of our calculation. The danger to be gua against is that of regarding it too purely as an algebr conception, and thence of sinking into the very nat errors both of too readily evolving it out of our own sciousness, and too freely pushing it to unwarranted leng

Or on the other hand, we may consider that we are tr ing of the behaviour of *things;*—balls, dice, births, de &c.; and drawing inferences about them. But, then, w were in the former instance allowable assumptions, bec here propositions to be tested by experience. Now the wl theory of Probability as a practical science, in fact as thing more than an algebraical truth, depends of course u there being a close correspondence between these two vi he subject, in other words, upon our substituted series g kept in accordance with the actual series. Experience ıdantly proves that, between considerable limits, in the nple in question, there does exist such a correspondence. let no one attempt to enforce our assent to every remote ıction that mathematicians can draw from their formulæ. en this is attempted the distinction just traced becomes ninent and important, and we have to choose our side. ıer we go over to the mathematics, and so lose all right iscussion about the things; or else we take part with the gs, and so defy the mathematics. We do not question formal accuracy of the latter within their own province, either we dismiss them as somewhat irrelevant, as applyto data of whose correctness we cannot be certain, or ake the liberty of remodelling them so as to bring them accordance with facts.

§ 13. A critic of any doctrine can hardly be considered ıave done much more than half his duty when he has ained and justified his grounds for objecting to it. It remains for him to indicate, if only in a few words, t he considers its legitimate functions and position to be, it can seldom happen that he regards it as absolutely thless or unmeaning. I should say, then, that when ɔability is thus divorced from direct reference to objects, ; substantially is by not being founded upon experience, mply resolves itself into the common algebraical or arithical doctrine of Permutations and Combinations[1]. The ıiderations upon which these depend are purely formal necessary, and can be fully reasoned out without any ɔal to experience. We there start from pure considera- s of number or magnitude, and we terminate with them,

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he close connection between subjects is well indicated in the title of Mr Whitworth's treatise, *Choice and Chance.*

having only arithmetical calculations to connect them t
gether. I wish, for instance, to find the chance[1] of throwi
heads three times running with a penny. All I have to d
is first to ascertain the possible number of throws. Perm
tations tell me that with two things thus in question (vi
head and tail) and three times to perform the process, th
are eight possible forms of the result. Of these eight o
only being favourable, the chance in question is pronounc
to be one-eighth.

Now though it is quite true that the actual calculatio
every chance problem must be of the above character, viz.
algebraical or arithmetical process, yet there is, it seems
me, a broad and important distinction between a mate
science which employs mathematics, and a formal one wh
consists of nothing but mathematics. When we cut o
selves off from the necessity of any appeal to experience,
are retaining only the intermediate or calculating part of t
investigation; we may talk of dice, or pence, or cards, b
these are really only names we choose to give to our symb
The H's and T's with which we deal have no bearing on o
jective occurrences, but are just like the x's and y's wi
which the rest of algebra deals. Probability in fact, when
treated, seems to be absolutely nothing else than a system
applied Permutations and Combinations.

It will now readily be seen how narrow is the range
cases to which any purely deductive method of treatment c
apply. It is almost entirely confined to such employme
as games of chance, and, as already pointed out, can only
regarded as really trustworthy even there, by the help
various tacit restrictions. This alone would be conclusi
against the theory of the subject being rested upon such
basis. The experimental method, on the other hand, is, in t
same theoretical sense, of universal application. It woul

ude the ordinary problems furnished by games of chance,
rell as those where the dice are loaded and the pence are
perfect, and also the indefinitely numerous applications
tatistics to the various kinds of social phenomena.

§ 14. The particular view of the deductive character of
bability above discussed, could scarcely have intruded
f into any other examples than those of the nature of
es of chance, in which the conditions of occurrence are
comparison few and simple, and are amenable to accurate
nerical determination. But a doctrine, which is in reality
le else than the same theory in a slightly disguised form,
ery prevalent, and has been applied to truths of the most
ely empirical character. This doctrine will be best in-
luced by a quotation from Laplace. After speaking of
irregularity and uncertainty of nature as it appears at
sight, he goes on to remark that when we look closer we
in to detect "a striking regularity which seems to sug-
a design, and which some have considered a proof of
vidence. But, on reflection, it is soon perceived that this
ularity is nothing but the development of the respective
abilities of the simple events, which ought to occur more
uently according as they are more probable[1]."

If this remark had been made about the succession of
ls and tails in the throwing up of a penny, it would
been intelligible. It would simply mean this: that the
titution of the body was such that we could anticipate
some confidence what the result would be when it was
ted in a certain way, and that experience would justify
anticipation in the long run. But applied as it is in a
general form to the facts of nature, it seems really to
but little meaning in it. Let us test it by an instance.
dst the irregularity of individual births, we find that the

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[1] *Essai Philosophique.* Ed. 1825, p. 74.

male children are to the female, in the long run, in s
the proportion of 106 to 100. Now if we were told
there is nothing in this but "the development of thei
spective probabilities," would there be anything in su
statement but a somewhat pretentious re-statement o
fact already asserted? The probability *is* nothing but
proportion, and is unquestionably in this case derived
no other source but the statistics themselves; in the
remark the attempt seems to be made to invert this pr
and to derive the sequence of events from the mere n
rical statement of the proportions in which they occur.

§ 15. It will very likely be replied that by the p
bility above mentioned is meant, not the mere num
proportion between the births, but some fact in our c
tution upon which this proportion depends; that ju
there was a relation of equality between the two sides o
penny, which produced the ultimate equality in the nu
of heads and tails, so there may be something in our c
tution or circumstances in the proportion of 106 to
which produces the observed statistical result. When
something, whatever it might be, was discovered, th
served numbers might be supposed capable of being
mined beforehand. Even if this were the case, howe
must not be forgotten that there could hardly fail to
combination with such causes, other concurrent conditi
order to produce the ultimate result; just as besides the
of the penny, we had also to take into account the nat
the 'randomness' with which it was tossed. What
may be, no one at present can undertake to say, for the
physiologists seem indisposed to hazard even a guess
the subject[1]. But without going into particulars, one

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[1] An opinion prevailed rather at one time (quoted and supported by Quetelet amongst others) th relative ages of the paren

t with some confidence that these conditions cannot well ltogether independent of the health, circumstances, man- and customs, &c. (to express oneself in the vaguest way) e parents; and if once these influencing elements are duced, even as very minute factors, the results cease to pendent only on fixed and permanent conditions. We t once letting in other conditions, which, if they also ss the characteristics that distinguish Probability (an dingly questionable assumption), must have that fact ally proved about them. That this should be the case ed seems not merely questionable, but almost certainly sible; for these conditions partaking of the nature of we term generally, Progress and Civilization, cannot be cted to show any permanent disposition to hover about verage.

§ 16. The reader who is familiar with Probability is of e acquainted with the celebrated theorem of James noulli. This theorem, of which the examples just ad- ed are merely particular cases, is generally expressed ewhat as follows:—in the long run all events will tend occur with a relative frequency proportional to their ctive probabilities. With the mathematical proof of this orem we need not trouble ourselves, as it lies outside the vince of this work; but indeed if there is any value in the going criticism, the basis on which the mathematics rest ulty, owing to there being really nothing which we can h propriety call an objective probability.

If one might judge by the interpretation and uses to

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thing to do with the sex of the ing. If this were so, it would bear out the above remarks. matter of fact, it should be red, that the proportion of 106 to 100 does not seem by any means universal in all countries or at all times. For various statistical tables on the subject see Quetelet, *Physique Sociale*, Vol. I. 166, 173, 238.

which this theorem is sometimes exposed, we should reg it as one of the last remaining relics of Realism, which a being banished elsewhere still manages to linger in the mote province of Probability. It would be an illustratio the inveterate tendency to objectify our conceptions, eve cases where the conceptions had no right to exist at all. uniformity is observed; sometimes, as in games of chanc is found to be so connected with the physical constitutio the bodies employed as to be capable of being inferred forehand; though even here the connection is by no m so necessary as is commonly supposed, owing to the fact in addition to these bodies themselves we have also to into account their relation to the agencies which influ them. This constitution is then converted into an 'objec probability', supposed to develope into the sequence wh exhibits the uniformity. Finally, this very questionable jective probability is assumed to exist, with the same fac of development, in all the cases in which uniformity is served, however little resemblance there may be betw these and games of chance.

§ 17. How utterly inappropriate any such conceptio in most of the cases in which we find statistical uniform will be obvious on a moment's consideration. The obser phenomena are generally the product, in these cases, of v numerous and complicated antecedents. The number crimes, for instance, annually committed in any society, i function amongst other things, of the strictness of the l the morality of the people, their social condition, and vigilance of the police, each of these elements being in it almost infinitely complex. Now, as a result of all th agencies, there is some degree of uniformity; but what been called above the change of type, which it sooner later tends to display, is unmistakeable. The average an mbers do not show a steady gradual approach towards at might be considered in some sense a limiting value, t, on the contrary, fluctuate in a way which, however it y depend upon causes, shows none of the permanent unimity which is characteristic of games of chance. This t, combined with the obvious arbitrariness of singling out, n amongst the many and various antecedents which proed the observed regularity, a few only, which should conute the objective probability (if we took all, the events g absolutely determined, there would be no occasion for appeal to probability in the case), would have been sufent to prevent any one from assuming the existence of y such thing, unless the mistaken analogy of other cases predisposed him to seek for it.

There is a familiar practical form of the same error, the dency to which may not improbably be derived from a ilar theoretical source. It is that of continuing to acculate our statistical data to an excessive extent. If the e were absolutely fixed we could not possibly have too ny statistics; the longer we chose to take the trouble collecting them the more accurate our results would be. t if the type is changing, in other words, if some of the ncipal causes which aid in their production have, in regard their present degree of intensity, strict limits of time or ce, we shall do harm rather than good if we overstep se limits. The danger of stopping too soon is easily seen, t in avoiding it we must not fall into the opposite error of ing on too long, and so getting either gradually or sudnly under the influence of a changed set of circumstances.

§ 18. This chapter was intended to be devoted to a sideration, not of the processes by which nature produces s series with which we are concerned, but of the theoretic is of the methods by which we can determine the existence of such series. But it is not possible to keep the two enqui apart, for here, at any rate, the old maxim prevails tha know a thing we must know its causes. Recur for a mi to the considerations of the last chapter. We there that there was a large class of events, the conditions of duction of which could be said to consist of (1) a comparati few nearly unchangeable elements, and (2) a vast nu of independent and very changeable elements. At le there were any other elements besides these, we are assu either to make special allowance for them, or to omit t from our enquiry. Now in certain cases, such as gam chance, the unchangeable elements may without prac error be regarded as really unchangeable throughout range of time and space. Hence, as a result, the deduc method of treatment becomes in their case at once the simple, natural, and conclusive; but, as a further consequ the statistics of the events, if we choose to appeal to t may be collected *ad libitum* with better and better proximation to truth. On the other hand, in all so applications of Probability, the unchangeable causes can be regarded as really unchangeable under many qualif tions. We know little or nothing of them directly; they often in reality numerous, indeterminate, and fluctuati and it is only under the guarantee of stringent restrictio time and place, that we can with any safety attribute them sufficient fixity to justify our theory. Hence, a result, the deductive method, under whatever name it go, becomes totally inapplicable both in theory and practi and, as a further consequence, the appeal to statistics ha be made with the caution in mind that we shall do misc rather than good if we go on collecting too many of them

§ 19. The results of the last two chapters may summed up as follows:—We have extended the concepti series obtained in the first chapter; for we have found these series are mostly presented to us in groups. e groups are found upon examination to be formed upon ximately the same type throughout a very wide and d range of experience; the causes of this agreement we ssed and explained in some detail. When, however, we d our examination by supposing the series to run to a great length, we find that they may be divided into two s separated by important distinctions. In one of these s (that containing the results of games of chance) the itions of production, and consequently the laws of statis- occurrence, may be practically regarded as absolutely ; and the extent of the divergences from the mean to know no finite limit. In the other class, on the ary (containing the bulk of ordinary statistical enquiries), onditions of production vary with more or less rapidity, so in consequence do the results. Moreover it is often ssible that variations from the mean should exceed a in amount. The former we may term *ideal* series. It ey alone which show the requisite characteristics with lose approach to accuracy, and to make the theory of ubject tenable, we have really to substitute one of this for one of the less perfect ones of the other class, when latter are under treatment. The former class have, ver, been too exclusively considered by writers on the ct; and conceptions appropriate only to them, and not ys even to them, have been imported into the other . It is in this way that a general tendency to an ex- ve deductive or *à priori* treatment of the science has encouraged.

CHAPTER V.

THE CONCEPTION RANDOMNESS *AND ITS SCIENT* *TREATMENT.*

§ 1. THERE is a term of frequent occurrence in t on Probability, and which we have already had repea casion to employ, viz. the designation *random* applie event, as in the expression 'a random distribution scientific conception involved in the correct use of th is, I apprehend, nothing more than that of aggregat and individual irregularity (or apparent irregularity) has been already described in the preceding chapt brief discussion of the requisites in this scientific con and in particular of the nature and some of the rea the departure from the popular conception, may s clear up some of the principal remaining difficulties attend this part of our subject.

The original[1], and still popular, signification of tl is of course widely different from the scientific. Y looks to is the origin, not the results, of the rando formance, and it has reference rather to the single than to a group or series of actions. Thus, when ws a bow 'at a venture', or 'at random', we mean only point out the aimless character of the performance; are contrasting it with the definite intention to hit a tain mark. But it is none the less true, as already inted out, that we can only apply processes of inference to ch performances as these when we regard them as being pable of frequent, or rather of indefinitely extended repeti- n.

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[1] According to Prof. Skeat (*Etymological Dictionary*) the earliest known meaning is that of *furious* action, as in a charge of cavalry. The etymology, he considers, is connected with the Teutonic w (brim), and implies the fu irregular action of a river f brim.

Begin with an illustration. Perhaps the best typical ample that we can give of the scientific meaning of ndom distribution is afforded by the arrangement of the ops of rain in a shower. No one can give a guess where- outs at any instant a drop will fall, but we know that if put out a sheet of paper it will gradually become uni- mly spotted over; and that if we were to mark out any o equal areas on the paper these would gradually tend be struck equally often.

§ 2. I. Any attempt to draw inferences from the as- mption of random arrangement must postulate the oc- rrence of this particular state of things at some stage or her. But there is often considerable difficulty, leading oc- sionally to some arbitrariness, in deciding the particular ge at which it ought to be introduced.

(1) Thus, in many of the problems discussed by mathe- aticians, we look as entirely to the results obtained, and ink as little of the actual process by which they are obtained, when we are regarding the arrangement of the drops of in. A simple example of this kind would be the following. pawn, diameter of base one inch, is placed at random on hess-board, the diameter of the squares of which is one inch d a quarter: find the chance that its base shall lie across e of the intersecting lines. Here we may imagine the wns to be so to say rained down vertically upon the board,

and the question is to find the ultimate proportion of th which meet a boundary line to the total of those which The problem therefore becomes a merely geometrical viz. to determine the ratio of a certain area on the boar the whole area. The determination of this ratio is all t the mathematician ever takes into account.

Now take the following. A straight brittle rod is bro at random in two places: find the chance that the pieces make a triangle[1]. Since the only condition for makin triangle with three straight lines is that each two shall greater than the third, the problem seems to involve same general conception as in the former case. We m conceive such rods breaking at one pair of spots a another,—no one can tell precisely where,—but showing same ultimate tendency to distribute these spots throug the whole length uniformly. As in the last case, the mat matician thinks of nothing but this final result, and pays heed to the process by which it may be brought about. cordingly the problem is again reduced to one of mensu tion, though of a somewhat more complicated character.

§ 3. (2) In another class of cases we have to contemp an intermediate process rather than a final result; but same conception has to be introduced here, though it is applied to the former stage, and in consequence will not general apply to the latter.

For instance: a shot is fired at random from a gun wh maximum range (i.e. at 45° elevation) is 3000 yards: wha the chance that the actual range shall exceed 2000 yar The ultimately uniform (or random) distribution here commonly assumed to apply to the various directions which the gun can be pointed; all possible directions ab

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[1] See the problem paper of Jan. 18, 1854, in the Cambridge Math tical Tripos.

horizontal being equally represented in the long run.
have therefore to contemplate a surface of uniform dis-
bution, but it will be the surface, not of the ground, but
a hemisphere whose centre is occupied by the man who
s. The ultimate distribution of the bullets on the spots
ere they strike the ground will not be uniform. The
blem is in fact to discover the law of variation of the
sity of distribution.

The above is, I presume, the treatment generally adopted
olving such a problem. But there seems no absolute
essity for any such particular choice. It is surely open to
one to maintain[1] that his conception of the randomness of
firing is assigned by saying that it is likely that a man
ld begin by facing towards any point of the compass in-
rently, and then proceed to raise his gun to any angle
fferently. The stage of ultimately uniform distribution
has receded a step further back. It is not assigned
ctly to the surface of an imaginary hemisphere, but to
lines of altitude and azimuth drawn on that surface.
cordingly, the distribution over the hemisphere itself will
now be uniform,—there will be a comparative crowding
towards the pole,—and the ultimate distribution over the
und will not be the same as before.

§ 4. Difficulties of this kind, arising out of the un-
tainty as to what stage should be selected for that of uni-
distribution, will occasionally present themselves. For
ance: let a book be taken at random out of a bookcase;
t is the chance of hitting upon some assigned volume?
ardly know how this question would commonly be treated.
e were to set our man opposite the middle of the shelf

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[1] As, according to Mr H. Godfray,
majority of the candidates *did*
me when the problem was once proposed in an examination. See the *Educational Times* (Reprint, Vol. VII. p. 99.)

and inquire what would generally happen in practice, s
posing him blindfolded, ~~there cannot be much doubt t~~
~~the volumes~~ would not be selected equally often. On
contrary, it is likely that there would be a tendency to
creased frequency about a centre indicated by the hei
of his shoulder, and (unless he be left-handed) a trifle to
right of the point exactly opposite his starting point.

If the question were one which it were really wo
while to work out on these lines we should be led a l
way back. Just as we imagined our rifleman's position
the second supposition) to be determined by two i
pendent coordinates of assumed continuous and equal faci
so we might conceive our making the attempt to analyse
man's movements into a certain number of indepen
constituents. We might suppose all the various direct
from his starting point, along the ground, to be equ
likely; and that when he rea~~ches the~~ shelves the ran
motion of his hand is to be regulated after the fashion
shot discharged at random.

The above would be one way of setting about the st
ment of the problem. But the reader will understand t
all which I am here proposing to maintain is that in th
as in every similar case, we al~~ways encounter,~~ under
conception of 'randomness', at some stage or other,
postulate of ultimate uniformity of distribution over
assigned magnitude: either time; or space, linear, superfi
or solid. But the selection of the stage at which this i
be applied may give rise to considerable difficulty, and e
arbitrariness of choice.

§ 5. Some years ago there was a very interesting dis
sion upon this subject carried on in the mathematical pa
the *Educational Times* (see, especially, Vol. VII.). As
unfrequently happens in mathematics there was an al

ire accord amongst the various writers as to the assump-
s practically to be made in any particular case, and there-
as to the conclusion to be drawn, combined with a very
iderable amount of difference as to the axioms and defi-
ons to be employed. Thus Mr M. W. Crofton, with the
stantial agreement of Mr Woolhouse, laid it down un-
itatingly that “at random” has “a very clear and definite
ning; one which cannot be better conveyed than by Mr
son's definition, ‘*according to no law*’; and in this sense
e I mean to use it.” According to any scientific inter-
tion of ‘law’ I should have said that where there was
aw there could be no inference. But ultimate tendency
rds equality of distribution is as much taken for granted
Mr Crofton as by any one else: in fact he makes this a
uction from his definition:—“As this infinite system of
llels are drawn according to no law, they are as thickly
osed along any part of the [common] perpendicular as
ng any other” (VII. p. 85). Mr Crofton holds that any
d of *unequal* distribution would imply law,—“If the points
a plane] tended to become denser in any part of the plane
n in another, there must be some law attracting them
re” (ib. p. 84). The same view is enforced in his paper
Local Probability (in the *Phil. Trans.*, Vol. 158). Surely
they tend to become *equally* dense this is just as much
se of regularity or law.

It may be remarked that wherever any serious practical
sequences turn upon duly securing the desired random-
s, it is always so contrived that no design or awkwardness
unconscious one-sidedness shall disturb the result. The
ncipal case in point here is of course afforded by games of
nce. What we want, when we toss a die, is to secure that
numbers from 1 to 6 shall be equally often represented in
long run, but that no person shall be able to predict the

individual occurrence. We might, in our statement o problem, as easily postulate 'a number *thought of* at rand as 'a shot fired at random', but no one would risk his cha of gain and loss on the supposition that this would be d with continued fairness. Accordingly, we construct a whose sides are accurately alike, and it is found that we do almost what we like with this, at any previous stag that of its issue from the dice box on to the table, wit interfering with the random nature of the result.

§ 6. II. Another characteristic in which the scien conception seems to me to depart from the popular or orig signification is the following. The area of distribution w we take into account must be a finite or limited one. necessity for this restriction may not be obvious at first s but the consideration of one or two examples will serv indicate the point at which it makes itself felt. Supp that one were asked to choose a number at random, from a finite range, but from the inexhaustible possibili of enumeration. In the popular sense of the term,—i. uttering a number without pausing to choose,—there is difficulty. But a moment's consideration will show that arrangement even tending towards ultimately uniform tribution can be secured in this way. No *average* could struck with ever increasing steadiness. So with sp infinity. We can rationally speak of choosing a point random in a given straight line, area, or volume. But if suppose the line to have no end, or the selection to be m in infinite space, the basis of ultimate tendency tow what may be called the equally thick deposit of our rand points fails us utterly.

Similarly in any other example in which one of magnitudes is unlimited. Suppose I fling a stick at rand in a horizontal plane against a row of iron railings quire for the chance of its passing through without touch- g them. The problem bears some analogy to that of the essmen, and so far as the motion of translation of the ck is concerned (if we begin with this) it presents no ficulty. But as regards the rotation it is otherwise. For y assigned linear velocity there is a certain angular ve- ity *below* which the stick may pass through without con- t, but *above* which it cannot. And inasmuch as the former nge is limited and the latter is unlimited, we encounter e ~~same impossibility as~~ before in ~~endeavouring~~ to conceive uniform distribution. Of course we might evade this rticular difficulty by beginning with an estimate of the gular velocity, when we should have to repeat what has t been said, mutatis mutandis, in reference to the linear locity.

§ 7. I am of course aware that there are a variety of oblems current which seem to conflict with what has just een said, but they will all submit to explanation. For in- ance; What is the chance that three straight lines, taken drawn at random, shall be of such lengths as will admit of eir forming a triangle? There are two ways in which we ay regard the problem. We may, for one thing, start with e assumption of three lines not greater than a certain ngth *n*, and then determine towards what limit the chance nds as *n* increases unceasingly. Or, we may maintain that e question is merely one of *relative proportion* of the three nes. We may then start with any magnitude we please to present one of the lines (for simplicity, say, the longest of em), and consider that all possible shapes of a triangle ill be represented by varying the lengths of the other two. either case we get a definite result without need to make attempt to conceive any random selection from the in- nity of possible length.

So in what is called the "three-point problem":—Th points in space are selected at random; find the chanc their forming an acute-angled triangle. What is done i start with a closed volume,—say a sphere, from its supe simplicity,—find the chance (on the assumption of unif distribution within this volume); and then conceive the c tinual enlargement without limit of this sphere. So rega the problem is perfectly consistent and intelligible, thoug fail to see why it should be termed a random selectio *space* rather than in a sphere. Of course if we started w a different volume, say a cube, we should get a differ result; and it is therefore contended (e.g. by Mr Crofton the *Educational Times*, as already referred to) that infi space is more naturally and appropriately regarded as ten towards by the enlargement of a sphere than by that o cube or any other figure.

Again: A group of integers is taken at random; sh that the number thus taken is more likely to be odd t even. What we do in answering this is to start with finite number n, and show that of all the possible co binations which can be made within this range there more odd than even. Since this is true irrespective of magnitude of n, we are apt to speak as if we could conc the selection being made at random from the true infi contemplated in numeration.

§ 8. Where these conditions cannot be secured the seems to me that the attempt to assign any finite value the probability fails. For instance, in the following probl proposed by Mr J. M. Wilson, "Three straight lines drawn at random on an infinite plane, and a fourth lin drawn at random to intersect them: find the probabilit its passing through the triangle formed by the other thr (*Ed. Times*, Reprint, Vol. v. p. 82), he offers the follow

lution: “Of the four lines, two must and two must not pass
thin the triangle formed by the remaining three. Since
l are drawn at random, the chance that the last drawn
uld pass through the triangle formed by the other three
consequently ½.”

I quote this solution because it seems to me to illustrate
difficulty to which I want to call attention. As the
oblem is worded, a triangle is supposed to be assigned by
ee straight lines. However large it may be, its size bears
finite ratio whatever to the indefinitely larger area out-
le it; and, so far as I can put any intelligible construction
the supposition, the chance of drawing a fourth random
e which should happen to intersect this finite area must
reckoned as zero. The problem Mr Wilson has solved
ems to me to be a quite different one, viz. “Given four
tersecting straight lines, find the chance that we should,
random, select one that passes through the triangle formed
the other three.”

The same difficulty seems to me to turn up in most other
tempts to apply this conception of randomness to real
finity. The following seems an exact analogue of the
ove problem:—A number is selected at random, find the
ance that another number selected at random shall be
ater than the former;—the answer surely must be that
chance is unity, viz. certainty, because the range above
assigned number is infinitely greater than that below it.
, expressed in the only language in which I can under-
nd the term ‘infinity’, what I mean is this. If the first
mber be m and I am restricted to selecting up to n
$> m$) then the chance of exceeding m is $n - m : n$; if I
restricted to $2n$ then it is $2n - m : 2n$ and so on. That
however large n and m may be the expression is always
elligible; but, m *being chosen first*, n may be made as

much larger than m as we please: i.e. the chance may made to approach as near to unity as we please.

I cannot but think that there is a similar fallacy in Morgan's admirably suggestive paper on *Infinity* (*C* *Phil. Trans.* Vol. 11.) when he is discussing the "three-p problem":—i.e. given three points taken at random find chance that they shall form an acute-angled triangle. that he shows is, that if we start with *one side as given* consider the subsequent possible positions of the opp vertex, there are infinitely as many such positions w would form an acute-angled triangle as an obtuse: but before, this is solving a different problem.

§ 9. The nearest approach I can make towards true in nite randomness, or random selection from true indefinite is as follows. Suppose a circle with a tangent line exte indefinitely in each direction. Now from the centre radii at random; in other words, let the semicircumfer which lies towards the tangent be ultimately uniformly tersected by the radii. Let these radii be then produced as to intersect the tangent line, and consider the distribut of these points of intersection. We shall obtain in the re *one* characteristic of our random distribution; i.e. no port of this tangent, however small or however remote, but find itself in the position ultimately of any small porti the pavement in our supposed continual rainfall. That any such elementary patch will become more and more cl dotted over with the points of intersection. But the o essential characteristic, viz. that of ultimately *uniform* tribution, will be missing. There will be a special form distribution,—what in fact will have to be discussed future chapter under the designation of a 'law of error',— virtue of which the concentration will tend to be greatest a certain point (that of contact with the circle), and will it from here in each direction according to an easily lculated formula. The existence of such a state of things s this is quite opposed to the conception of true randomness.

§ 10. III. Apart from definitions and what comes of hem, perhaps the most important question connected with he conception of Randomness is this: How in any given se are we to determine whether an observed arrangement to be considered a random one or not? This question will ve to be more fully discussed in a future chapter, but we e already in a position to see our way through some of the fficulties involved in it.

(1) If the events or objects under consideration are supsed to be continued indefinitely, or if we know enough out the mode in which they are brought about to detect ir ultimate tendency,—or even, short of this, if they are merous enough to be beyond practical counting,—there is great difficulty. We are simply confronted with a queson of fact, to be settled like other questions of fact. In the se of the rain-drops, watch two equal squares of pavement other surfaces, and note whether they come to be more d more densely uniformly and evenly spotted over: if they then the arrangement is what we call a random one. If want to know whether a tobacco-pipe really breaks at ranm, and would therefore serve as an illustration of the oblem proposed some pages back, I have only to drop ough of them and see whether pieces of all possible lengths e equally represented in the long run. Or, I may argue ductively, from what I know about the strength of marials and the molecular constitution of such bodies, as to hether fractures of small and large pieces are all equally kely to occur.

§ 11. The reader's attention must be carefully directed a source of confusion here, arising out of a certain cross-division. What we are now discussing is a question of fa viz. the nature of a certain ultimate arrangement; we a not discussing the particular way in which it is broug about. In other words, the antithesis is between what is a what is not random: it is not between what is random a what is designed. As we shall see in a few moments it quite possible that an ~~arrangement which is the result,—~~ ever anything were so,—of 'design', may nevertheless prese ~~the unmistakeable stamp of~~ randomness of arrangement.

Consider a case which has been a good deal discuss and to which we shall revert again: ~~the arrangement of t~~ stars. ~~The~~ question here is rather complicated by the fa that we know nothing about the actual mutual positions the stars, all that we can take cognizance of being their a parent or visible places as projected upon the surface of supposed sphere. Appealing to what alone we can th observe, it is obvious that the arrangement, as a whole, not of the random sort. The Milky Way and the other r solvable nebulæ, as they present themselves to us, are as o vious an infraction of such an arrangement as would be th occurrence here and there of patches of ground in a rain-f which received a vast number more drops than the spac surrounding them. If we leave these exceptional areas o of the question and consider only the stars which are visib by the naked eye or by slight telescopic power, it seem equally certain that the arrangement *is*, for the most part, fairly representative random one. By this we mean nothi more than ~~the fact~~ that when we ~~mark~~ off any number equal areas on the visible sphere these are found to conta approximately the same number of ~~stars.~~

The actual arrangement of the stars in space *may* a be of the same character: that is, the apparently dem aggregation may be apparent only, arising from the fact th are looking through regions which are not more thickly upied but are merely more extensive. The alternative fore us, in fact, is this. If the whole volume, so to say, of e starry heavens is tolerably regular in shape, then the rangement of the stars is not of the random order; if that lume is very irregular in shape, it is possible that the arngement within it may be throughout of that order.

§ 12. (2) When the arrangement in question includes it a comparatively small number of events or objects, it comes much more difficult to determine whether or not it to be designated a random one. In fact we have to shift r ground, and to decide not by what has been actually served but by what we have reason to conclude would be served if we could continue our observation much longer. his introduces what is called 'Inverse Probability', viz. the termination of the nature of a cause from the nature of the bserved effect; a question which will be fully discussed in future chapter. But some introductory remarks may be onveniently made here.

Every problem of Probability, as the subject is here underood, introduces the conception of an ultimate limit, and erefore presupposes an indefinite possibility of repetition. hen we have only a finite number of occurrences before us, *irect* evidence of the character of their arrangement fails us, d we have to fall back upon the nature of the agency hich produces them. And as the number becomes smaller e confidence with which we can estimate the nature of the gency becomes gradually less.

Begin with an intermediate case. There is a small lawn, rinkled over with daisies: is this a random arrangement? e feel some confidence that it is so, on mere inspection; eaning by this that (negatively) no trace of any regular ttern can be discerned and (affirmatively) that if we take any moderately small area, say a square yard, we shall much about the same number of the plants included i But we can help ourselves by an appeal to the known ag of distribution here. We know that the daisy spread seed, and considering the effect of the wind and the conti sweeping and mowing of the lawn we can detect cause work which are analogous to those by which the dealin cards and the tossing of dice are regulated.

In the above case the appeal to the process of produc was subsidiary, but when we come to consider the natu a very small succession or group this appeal becomes n more important. Let us be told of a certain successio 'heads' and 'tails' to the number of ten. The range he far too small for decision, and unless we are told whether agent who obtained them was tossing or designing we quite unable to say whether or not the designation of ' dom' ought to be applied to the result obtained. The t must never be forgotten that though 'design' is sure break down in the long run if it make the attempt to duce directly the semblance of ~~randomness[1], yet for a s~~ spell it can simulate it perfectly. Any short succession, of heads and tails, may have been equally well brou about by tossing or by deliberate choice.

§ 13. The reader will observe that this ~~questio~~ randomness is being here treated as simply one of ultin ~~statistical fact.~~ I have fully admitted that this ~~is not~~ primitive conception, nor is it the popular interpreta but to adopt it seems the only course open to us if we ar draw inferences such as those contemplated in Probabi When we look to the producing agency of the ultin arrangement we may find this very various. It may p itself to be (a few stages back) one of conscious delibe pose, as in drawing a card or tossing a die: it may be outcome of an extremely complicated interaction of many ural causes, as in the arrangement of the flowers scattered r a lawn or meadow: it may be of a kind of which we ow literally nothing whatever, as in the case of the actual angement of the stars relatively to each other.

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[1] Vide p. 68.

This was the state of things had in view when it was d a few pages back that randomness and design would ult in something of a cross-division. Plenty of arrangents in which design had a hand, a stage or two back, can mentioned, which would be quite indistinguishable in ir results from those in which no design whatever could traced. Perhaps the most striking case in point here is be found in the arrangement of the digits in one of the tural arithmetical constants, such as π or ϵ, or in a table logarithms. If we look to the process of production of se digits, no extremer instance can be found of what we an by the antithesis of randomness: every figure has its cessarily pre-ordained position, and a moment's flagging of ention would defeat the whole purpose of the calculator. d yet, if we look to results only, no better instance can found than one of these rows of digits if it were intended illustrate what we practically understand by a chance angement of a number of objects. Each digit occurs proximately equally often, and this tendency developes as advance further; the mutual juxtaposition of the digits o shows the same tendency, that is, any digit (say 5) is t as often followed by 6 or 7 as by any of the others. In t, if we were to take the whole row of hitherto calculated ures, cut off the first five as familiar to us all, and connplate the rest, no one would have the slightest reason uppose that these had not come out as the results of a with ten equal faces.

§ 14. If it be asked *why* this is so, a rather puz question is raised. Wherever physical causation is inv we are generally understood to have satisfied the de implied in this question if we assign antecedents which be followed regularly by the event before us; but in geon and arithmetic there is no opening for antecedents. we then commonly look for is a demonstration, i.e. th solution of the observed fact into axioms if possible, any rate into admitted truths of wider generality. I d know that a demonstration can be given as to the exis of this characteristic of statistical randomness in such cessions of digits as those under consideration. Bu following remarks may serve to shift the onus of unl hood by suggesting that the preponderance of analo rather in favour of the existence.

Take the well-known constant π for consideration. stands for a quantity which presents itself in a vast nu of arithmetical and geometrical relations; let us tal examination the best known of these, by regarding standing for the ratio of the circumference to the dia of a circle. So regarded, it is nothing more than a s case of the measurement of a magnitude by an arbit selected unit. Conceive then that we had before us or line and that we wished to measure it with ab accuracy. We must suppose—if we are to have a su analogue to the determination of π to several hu figures,—that by the application of continued higher m fying power we can detect ever finer subdivisions i graduation. We lay our rod against the scale and fi say, fall between 31 and 32 inches; we then look a next division of the scale, viz. that into tenths of an Can we see the slightest reason why the number of tenths should be other than independent of the numb ole inches? The "piece over" which we are measuring y in fact be regarded as an entirely new piece, which had len into our hands after that of 31 inches had been asured and done with; and similarly with every successive ice over, as we proceed to the ever finer and finer divisions.

Similar remarks may be made about most other incommsurable quantities, such as irreducible roots. Conceive o straight lines at right angles, and that we lay off a tain number of inches along each of these from the point intersection; say two and five inches, and join the exmities of these so as to form the diagonal of a right-angled angle. If we proceed to measure this diagonal in terms either of the other lines we are to all intents and purposes tracting a square root. We should expect, rather than erwise, to find here, as in the case of π, that incommenability and resultant randomness of order in the digits s the rule, and commensurability was the exception. Now d then, as when the two sides were three and four, we uld find the diagonal commensurable with them; but se would be the occasional exceptions, or rather they uld be the comparatively finite exceptions amidst the lefinitely numerous cases which furnished the rule.

§ 15. The best way perhaps of illustrating the truly dom character of such a row of figures is by appealing to phical aid. It is not easy here, any more than in ordinary tistics, to grasp the import of mere figures; whereas the angement of groups of points or lines is much more dily seized. The eye is very quick in detecting any nptoms of regularity in the arrangement, or any tendency denser aggregation in one direction than in another. w then are we to dispose our figures so as to force them display their true character? I should suggest that we about *drawing a line at random;* and, since we cannot trust our own unaided efforts to do this, that we rely u the help of such a table of figures to do it for us, and t examine with what sort of efficiency they can perform task. The problem of drawing *straight* lines at rand under various limitations of direction or intersectio familiar enough, but I do not know that any one has s gested the drawing of a line whose shape as well as posi shall be of a purely random character. For simplicity suppose the line to be confined to a plane.

The definition of such a line does not seem to inv any particular difficulty. Phrased in accordance with t ordinary language we should describe it as the path (i.e. t path) traced out by a point which at every moment is likely to move in any one direction as in any other. T we could not ourselves draw such a line, and that we ca not get it traced by any physical agency, is certain. T mere inertia of any moving body will always give i tendency, however slight, to go on in a straight line at e moment, instead of being instantly responsive to insta neously varying dictates as to its direction of motion. Nor we conceive or picture such a line in its ultimate or ideal c dition. But it is easy to give a graphical approximation t and it is easy also to show how this approximation ma carried on as far as we please towards the ideal in questi

We may proceed as follows. Take a sheet of the ordi ruled paper prepared for the graphical exposition of cu Select as our starting point the intersection of two of t lines, and consider the eight 'points of the compass' dicated by these lines and the bisections of the conta right angles[1]. For suggesting the random selection amo ese directions let them be numbered from 0 to 7, and t us say that a line measured due 'north' shall be de- gnated by the figure 0, 'north-east' by 1, and so on. The ection amongst these numbers, and therefore directions, at ery corner, might be handed over to a die with eight faces; t for the purpose of the illustration in view we select the gits 0 to 7 as they present themselves in the calculated lue of π. The sort of path along which we should vel by a series of such steps thus taken at random y be readily conceived; it is given at the end of this pter.

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[1] It would of course be more complete to take *ten* alternatives of direction, and thus to omit none of the digits; but this is much mor blesome in practice than to c ourselves to eight.

For the purpose with which this illustration was pro- sed, viz. the graphical display of the succession of digits any one of the incommensurable constants of arithmetic geometry, the above may suffice. After actually testing e of them in this way they seem to me, so far as the eye, the theoretical principles to be presently mentioned, are y guide, to answer quite fairly to the description of ran- mness.

§ 16. As we are on the subject, however, it seems worth ing farther by enquiring how near we could get to the eal of randomness of direction. To carry this out com- etely two improvements must be made. For one thing, tead of confining ourselves to eight directions we must mit an infinite number. This would offer no great diffi- lty; for instead of employing a small number of digits we uld merely have to use some kind of circular teetotum ich would rest indifferently in any direction. But in the t place instead of short finite steps we must suppose them efinitely short. It is here that the actual unattainability kes itself felt. We are familiar enough with the device, ployed by Newton, of passing from the discontinuous lygon to the continuous curve. But we can resort to this device because the ideal, viz. the curve, is as easily dra (and, I should say, as easily conceived or pictured) as any the steps which lead us towards it. But in the case be us it is otherwise. The line in question will remain continuous, or rather angular, to the last: for its angles not tend even to lose their sharpness, though the fragme which compose them increase in number and diminish magnitude without any limit. And such an ideal is not ceivable as an ideal. It is as if we had a rough body un the microscope, and found that as we subjected it to hig and higher powers there was no tendency for the angles round themselves off. Our 'random line' must remain 'spiky' as ever, though the size of its spikes of co diminishes without any limit.

The case therefore seems to be this. It is easy, in wo to indicate the conception by speaking of a line which every instant is as likely to take one direction as anot It is easy moreover to draw such a line with any deg of minuteness which we choose to demand. But it is possible to conceive or picture the line in its ultimate fo There is in fact no 'limit' here, intelligible to the un standing or picturable by the imagination (corresponding the asymptote of a curve, or the continuous curve to incessantly developing polygon), towards which we find selves continually approaching, and which therefore we apt to conceive ourselves as ultimately attaining The u assumption therefore which underlies the Newtonian finitesimal geometry and the Differential Calculus, cease apply here.

§ 17. If we like to consider such a line in one of approximate stages, as above indicated, it seems to me

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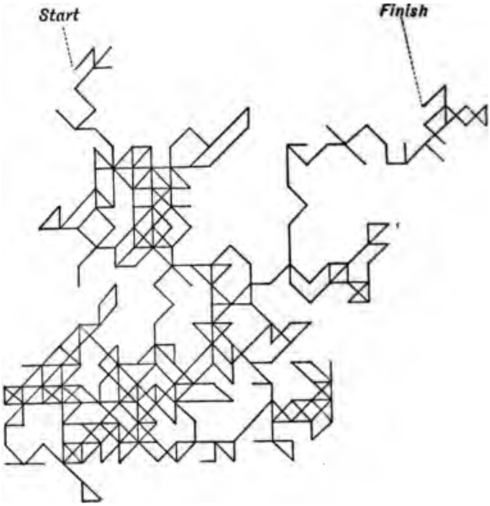
[1] Any more than we picture the shape of an equiangular spiral *centre.*

ne of the usual theorems of Probability, where large mbers are concerned, may safely be applied. If it be ed, for instance, whether such a line will ultimately tend stray indefinitely far from its starting point, Bernoulli's aw of Large Numbers' may be appealed to, in virtue of ich we should say that it was excessively unlikely that its vergence should be relatively great. Recur to our graical illustration, and consider first the resultant deviation the point (after a great many steps) right or left of the rtical line through the starting point. Of the eight adssible motions at each stage two will not affect this relative ition, whilst the other six are equally likely to move us a ep to the right or to the left. Our resultant 'drift' therere to the right or left will be analogous to the resultant fference between the number of heads and tails after a eat many tosses of a penny. Now the well-known outme of such a number of tosses is that ultimately the *oportional* approximation to the à priori probability, i.e. to uality of ~~heads and tails,~~ is more and more nearly ~~carried~~ ~~t, but that the *absolute* deflection is more~~ and more widely ~~played.~~

Applying this to the case in point, and remembering at the results apply equally to the horizontal and vertical rections, we should say that after any very great number such 'steps' as those contemplated, the ratio of our disnce from the starting point to the whole distance travelled ll pretty certainly be small, whereas the actual distance m it would be large. We should also say that the longer e continued to produce such a line the more pronounced uld these tendencies become. So far as concerns this test, d that afforded by the general appearance of the lines awn,—this last, as above remarked, being tolerably trustrthy,—I feel no doubt as to the generally 'random' character of the rows of figures displayed by the incomme surable or irrational ratios in question.

As it may interest the reader to see an actual specim of such a path I append one representing the arrangem of the eight digits from 0 to 7 in the value of π. The d are taken from Mr Shanks' astonishing performance in t calculation of this constant to 707 places of figures (*Pro R. S.*, XXI. p. 319). Of these, after omitting 8 and 9, th remain 568; the diagram represents the course traced o by following the direction of these as the clue to our pa Many of the steps have of course been taken in oppo directions twice or oftener. The result seems to me furnish a very fair graphical indication of randomness have compared it with corresponding paths furnished rows of figures taken from logarithmic tables, and in ot ways, and find the results to be much the same.



CHAPTER VI[1].

THE SUBJECTIVE SIDE OF PROBABILITY. MEASUREMENT OF BELIEF.

1. HAVING now obtained a clear conception of a n kind of series, the next enquiry is, What is to be with this series? How is it to be employed as a means king inferences? The general step that we are now to take might be described as one from the objective subjective, from the things themselves to the state of inds in contemplating them.

ıe reader should observe that a substitution has, in a number of cases, already been made as a first stage ds bringing the things into a shape fit for calculation. ubstitution, as described in former chapters, is, in a re, a process of *idealization*. The series we actually with are apt to show a changeable type, and the indivi- of them will sometimes transgress their licensed irregu- Hence they have to be pruned a little into shape, as

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ginally written in somewhat irit of protest against what to me the prevalent dis- to follow De Morgan in too subjective a view of the In reading it through now t find any single sentence to could take distinct objection, I must admit that if I were it entirely afresh I should endeavour to express myself with less emphasis, and I have made alterations in that direction. The reader who wishes to see a view not substantially very different from mine, but expressed with a somewhat opposite emphasis, can refer to Mr F. Y. Edgeworth's article on "The Philosophy of Chance" (*Mind*, Vol. IX.)

natural objects almost always have before they are capable being accurately reasoned about. The form in which th series emerges is that of a series with a fixed type. Th imaginary or ideal series is the basis of our calculation.

§ 2. It must not be supposed that this is at all at va ance with the assertion previously made, that Probability is science of inference about real things; it is only by a sub tution of the above kind that we are enabled to reason ab the things. In nature nearly all phenomena present the selves in a form which departs from that rigorously accur one which scientific purposes mostly demand, so we have introduce an imaginary series, which shall be free from such defects. The only condition to be fulfilled is, that th substitution is to be as little arbitrary, that is, to vary fr the truth as slightly, as possible. This kind of substituti generally passes without notice when natural objects of kind are made subjects of exact science. I direct disti attention to it here simply from the apprehension that wa of familiarity with the subject-matter might lead some read to suppose that it involves, in this case, an exceptional flection from accuracy in the formal process of inference.

It may be remarked also that the adoption of this ima nary series offers no countenance whatever to the doctri criticised in the last chapter, in accordance with which it w supposed that our series possessed a fixed unchangeable ty which was merely the "development of the probabilities" things, to use Laplace's expression. It differs from anythi contemplated on that hypothesis by the fact that it is to recognized as a necessary substitution of our own for th actual series, and to be kept in as close conformity with facts possible. It is a mere fiction or artifice necessarily resort to for the purpose of calculation, and for this purpose only.

This caution is the more necessary, because in the examp at I shall select, and which belongs to the most favourite ass of examples in this subject, the substitution becomes cidentally unnecessary. The things, as has been repeatedly ointed out, may sometimes need no trimming, because in e form in which they actually present themselves they *are* most idealized. In most cases a good deal of alteration is ecessary to bring the series into shape, but in some—proinently in the case of games of chance—we find the alterions, for all practical purposes, needless.

§ 3. We start then, from such a series as this, upon the nquiry, What kind of inference can be made about it? It y assist the logical reader to inform him that our first step ll be analogous to one class of what are commonly known *immediate* inferences,—inferences, that is, of the type,— ll men are mortal, therefore any particular man or men e mortal.' This case, simple and obvious as it is in Logic, quires very careful consideration in Probability.

It is obvious that we must be prepared to form an opinion pon the propriety of taking the step involved in making ch an inference. Hitherto we have had as little to do as ssible with the irregular individuals; we have regarded em simply as fragments of a regular series. But we cannot g continue to neglect all consideration of them. Even if ese events in the gross be tolerably certain, it is not only the gross that we have to deal with them; they conantly come before us a few at a time, or even as indiviuals, and we have to form some opinion about them in this ate. An insurance office, for instance, deals with numrs large enough to obviate most of the uncertainty, but ch of their transactions has another party interested in —What has the man who insures to say to their proceedgs? for to him this question becomes an individual one. d even the office itself receives its cases singly, and would therefore like to have as clear views as possible about th single cases. Now, the remarks made in the preceding chapt about the subjects which Probability discusses might seem t preclude all enquiries of this kind, for was not ignorance the individual presupposed to such an extent that even (will be seen hereafter) causation might be denied, with considerable limits, without affecting our conclusions? Th answer to this enquiry will require us to turn now to t consideration of a totally distinct side of the question, a one which has not yet come before us. Our best introducti to it will be by the discussion of a special example.

§ 4. Let a penny be tossed up a very great m times; we may then be supposed to know for certain th fact (amongst many others) that in the long run head a tail will occur about equally often. But suppose we consid only a moderate number of throws, or fewer still, and continue limiting the number until we come down to thr or two, or even one? We have, as the extreme cases, ce tainty or something undistinguishably near it, and ut uncertainty. Have we not, between these extremes, gradations of belief? There is a large body of writers, i cluding some of the most eminent authorities upon th subject, who state or imply that we are ~~distinctly consciou~~ such a variation of the amount of our belief, and ~~that~~ t state of our minds can be measured ~~and determined~~ w almost the same accuracy as the external events to whi they refer. The principal mathematical supporter of t view is De Morgan, who has insisted strongly upon it in his works on the subject. The clearest exposition of h opinions will be found in his *Formal Logic*, in which work has made the view which we are now discussing the basis his system. He holds that we have a certain amount of be of every proposition which may be set before us, an amou ich in its nature admits of determination, though we may ctically find it difficult in any particular case to determine He considers, in fact, that Probability is a sort of sister ence to Formal Logic[1], speaking of it in the following rds: "I cannot understand why the study of the effect, ich partial belief of the premises produces with respect to conclusion, should be separated from that of the conuences of supposing the former to be absolutely true[2]". other words, there is a science—Formal Logic—which instigates the rules according to which one proposition can be cessarily inferred from another; in close correspondence h this there is a science which ~~investigates~~ the rules acding to which ~~the amount~~ of our ~~belief of one~~ proposition ~~ies with~~ the amount of our belief of other propositions h which it is connected.

The same view is also supported by another high authority, late Prof. Donkin, who says (*Phil. Mag.* May, 1851), "It ll, I suppose, be generally admitted, and has often been re or less explicitly stated, that the subject-matter of culation in the mathematical theory of Probabilities is *antity of belief.*"

§ 5. Before proceeding to criticise this opinion, one rerk may be made upon it which has been too frequently rlooked. It should be borne in mind that, even were this w of the ~~subject not~~ actually incorrect, it might be objected as insufficient for the purpose of a definition, on the ground t variation of belief is not confined to Probability. It is property with which that science is concerned, no doubt, t it is a property which meets us in other directions as well. In every case in which we extend our inferences by Induction or Analogy, or depend upon the witness of others, or trust to our own memory of the past, or come to a conclusion through conflicting arguments, or even make a long and complicated deduction by mathematics or logic, we have a result of which we can scarcely feel as certain as of the premises from which it was obtained. In all these cases then we are conscious of varying quantities of belief, but are the laws according to which the belief is produced and varied the same? If they cannot be reduced to one harmonious scheme, if in fact they can at best be brought to nothing but a number of different schemes, each with its own body of laws and rules, then it is vain to endeavour to force them into one science.

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[1] In the ordinary signification of term. As De Morgan uses it akes Formal Logic *include* Proility, as one of its branches, as indicated in his title "Formal Logic, or the Calculus of Inference, necessary and probable."

[2] *Formal Logic.* Preface, page v.

This opinion is strengthened by observing that most of the writers who adopt the definition in question do practically dismiss from consideration most of the above-mentioned examples of diminution of belief, and confine their attention to classes of events which have the property discussed in Chap. I., viz. 'ignorance of the few, knowledge of the many.' It is quite true that considerable violence has to be done to some of these examples, by introducing exceedingly arbitrary suppositions into them, before they can be forced to assume a suitable form. But still there is little doubt that, if we carefully examine the language employed, we shall find that in almost every case assumptions are made which virtually imply that our knowledge of the individual is derived from propositions given in the typical form described in Chap. I. This will be more fully proved when we come to consider some common misapplications of the science.

§ 6. Even then, if the above-mentioned view of the subject were correct, it would yet, I consider, be insufficient for the purpose of a definition; but it is at least very doubtful whether it is correct. Before we could properly assign to e belief side of the question the prominence given to it by e Morgan and others, certainly before the science could be fined from that side, it would be necessary, it appears, to tablish the two following positions, against both of which rong objections can be brought.

(1) That our belief of every proposition is a thing which we can, strictly speaking, be said to measure; that there must be a certain amount of it in every case, which we can realize somehow in consciousness and refer to some standard so as to pronounce upon its value.

(2) That the value thus apprehended is the correct one according to the theory, viz. that it is the exact fraction of full conviction that it should be. This statement will perhaps seem somewhat obscure at first; it will be explained presently.

§ 7. (I) Now, in the first place, as regards the difficulty f obtaining any measure of the amount of our belief. One urce of this difficulty is too obvious to have escaped notice; his is the disturbing influence produced on the quantity of elief by any strong emotion or passion. A deep interest in he matter at stake, whether it excite hope or fear, plays great avoc with the belief-meter, so that we must assume the ind to be quite unimpassioned in weighing the evidence. his is noticed and acknowledged by Laplace and others; ut these writers seem to me to assume it to be the only urce of error, and also to be of comparative unimportance. ven if it were the only source of error I cannot see that it ould be unimportant. We experience hope or fear in so ry many instances, that to omit such influences from consideration would be almost equivalent to saying that whilst e profess to consider the whole quantity of our belief we ill in reality consider only a portion of it. Very strong

feelings are, of course, exceptional, but we should never
less find that the emotional element, in some form or ot
makes itself felt on almost every occasion. It is very sel
that we cannot speak of our surprise or expectation in r
ence to any particular event. Both of these expressions,
especially the former, seem to point to something more t
mere belief. It is true that the word 'expectation' is g
rally defined in treatises on Probability as equivalent
belief; but it seems doubtful whether any one who att
to the popular use of the terms would admit that they
exactly synonymous. Be this however as it may, the e
tional element is present upon almost every occasion, an
disturbing influence therefore is constantly at work.

§ 8. Another cause, which co-operates with the for
is to be found in the extreme complexity and variety of
evidence on which our belief of any proposition depe
Hence it results that our actual belief at any given mom
is one of the most fugitive and variable things possibl
that we can scarcely ever get sufficiently clear hold of i
measure it. This is not confined to the times when
minds are in a turmoil of excitement through hope or
In our calmest moments we shall find it no easy thin
give a precise answer to the question, How firmly do I
this or that belief? There may be one or two promi
arguments in its favour, and one or two corresponding
jections against it, but this is far from comprising all
causes by which our state of belief is produced. Bec
such reasons as these are all that can be practically in
duced into oral or written controversies, we must not
clude that it is by these only that our conviction is influen
On the contrary, our conviction generally rests upon a
of chaotic basis composed of an infinite number of infere
and analogies of every description, and these moreover

rted by our state of feeling at the time, dimmed by the gree of our recollection of them afterwards, and probably eived from time to time with varying force according to e way in which they happen to combine in our conscious-ss at the moment. To borrow a striking illustration from braham Tucker, the substructure of our convictions is not much to be compared to the solid foundations of an ordi-ry building, as to the piles of the houses of Rotterdam ich rest somehow in a deep bed of soft mud. They bear eir weight securely enough, but it would not be easy to int out accurately the dependence of the different parts pon one another. Directly we begin to think of the amount f our belief, we have to think of the arguments by which it produced—in fact, these arguments will intrude themselves thout our choice. As each in turn flashes through the nd, it modifies the strength of our conviction; we are like person listening to the confused hubbub of a crowd, where here is always something arbitrary in the particular sound e choose to listen to. There may be reasons enough to uffice abundantly for our ultimate choice, but on examina-on we shall find that they are by no means apprehended ith the same force at different times. The belief produced y some strong argument may be very decisive at the mo-ent, but it will often begin to diminish when the argument s not actually before the mind. It is like being dazzled by strong light; the impression still remains, but begins al-ost immediately to fade away. I think that this is the se, however we try to limit the sources of our conviction.

§ 9. (II) But supposing that it were possible to strike sort of average of this fluctuating state, should we find this erage to be of the amount assigned by theory? In other ords, is our natural belief in the happening of two different ents in direct proportion to the frequency with which those events happen in the long run? There is a lottery with tickets and ten prizes; is a man's belief that he will g prize fairly represented by one-tenth of certainty? The reference to a lottery should be sufficient to disprove Lotteries have flourished at all times, and have never f to be abundantly supported, in spite of the most perfect viction, on the part of many, if not of most, of those who into them, that in the long run all will lose. Deduct should undoubtedly be made for those who act from su stitious motives, from belief in omens, dreams, and so But apart from these, and supposing any one to come f fied by all that mathematics can do for him, it is difficul believe that his natural impressions about single events w be always what they should be according to theory. there many who can honestly declare that they would l no desire to buy a single ticket? They would probably to themselves that the sum they paid away was not worth mentioning to lose, and that there was a chanc gaining a great deal; in other words, they are not ap tioning their belief in the way that theory assigns.

What bears out this view is, that the same persons would act in this way in single instances would often think of doing so in any but single instances. In o words, the natural tendency here is to attribute too grea amount of belief where it is or should be small; i.e. to preciate the risk in proportion to the contingent advant They would very likely, when argued with, attach dispa ing epithets to this state of feeling, by calling it an accountable fascination, or something of that kind, bu its existence there can be little doubt. We are speak now of what is the natural tendency of our minds, n that into which they may at length be disciplined by ed tion and thought. If, however, educated persons have l for the most part in controlling this tendency in of chance, the spirit of reckless speculation has ly yet been banished from commerce. On examination, endency will be found so prevalent in all ages, ranks, ispositions, that it would be inadmissible to neglect it in to bring our supposed instincts more closely into acice with the commonly received theories of Probability.

10. There is another aspect of this question which has often overlooked, but which seems to deserve some ion. Granted that we have an instinct of credence, hould it be assumed that this must be just of that iny which subsequent experience will justify? Our cts are implanted in us for good purposes, and are ind to act immediately and unconsciously. They are, rer, subject to control, and have to be brought into lance with what we believe to be true and right. In departments of psychology we do not assume that spontaneous prompting of nature is to be left just as id it, or even that on the average, omitting individual ions, it is set at that pitch that will be found in the be the best when we come to think about it and assign les. Take, for example, the case of resentment. Here ve an instinctive tendency, and one that on the whole d in its results. But moralists are agreed that almost r efforts at self-control are to be directed towards sub it and keeping it in its right direction. It is assumed given as a sort of rough protection, and to be set, if night so express oneself, at too high a pitch to be rately and consciously acted on in society. May not hing of this kind be the case also with our belief? make a passing reference to this point here, as on eory of Probability adopted in this work it does not r to be at all material to the science. But it seems

a strong argument ~~against the expediency~~ of commei the study of the ~~science from the subjective side, or evi assigning any great degree of prominence to this side~~.

That men *do* not believe in exact accordance with theory must have struck almost every one, but this probably been considered as mere exception and irregula the assumption being made that on the average, and ii the majority of cases, they do so believe. As stated al it is very doubtful whether the tendency which has been discussed is not so widely prevalent that it might far more propriety be called the rule than the excep And it may be better that this should be so: many results may follow from that cheerful disposition whicl duces a man sometimes to go on trying after some g good, the chance of which he overvalues. He will keej through trouble and disappointment, without serious l perhaps, when the cool and calculating bystander sees pl that his 'measure of belief' is much higher than it sh be. So, too, the tendency also so common, of underra the chance of a great evil may also work for good. By n men death might be looked upon as an almost infinite at least they would so regard it themselves; suppose kept this contingency constantly before them at its i value, how would it be possible to get through the prac work of life? Men would be stopping indoors becau they went out they might be murdered or bitten by a dog. To say this is not to advocate a return to our insti indeed when we have once reached the critical and cons state, it is hardly possible to do so; but it should be no that the advantage gained by correcting them is at bes a balanced one[1]. What is most to our present purpo

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[1] An illustration of the points here insisted on has recently [1876] been given in a quarter whe would have expected it; I all

~~the in~~expediency of attempting ~~to found an~~ exact n ~~what may afterwards prove to be a mere instinct,~~ rized in its full extent by experience.

~~It may be replied, that tho~~ugh people, as a matter do not apportion belief in this exact way, yet they do so. The purport of this remark will be examined y; it need only be said here that it grants all that ontended for. For it admits that the degree of our capable of modification, and ~~may need it. But in~~ ıce with what is the belief to be modified? obviously dance with experience; it cannot be trusted to by ıt the fraction at which it is to be rated must be ıed by the comparative frequency ~~of the events to~~ refers. Experience then furnishing the standard, it ~~most reasonable to start from this~~ experience, and to ıe theory of our processes upon it.

e do not do this, it should be observed that we are ıg Probability altogether from the study of things to us, and making it nothing else in effect than a of Psychology. If we refuse to be controlled by ıce, but confine our attention to the laws according ı belief is naturally or instinctively compounded and ted in our minds, we have no right then to appeal to ıce afterwards even for illustrations, unless under the

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ders will readily infer, to l's exceedingly interesting Theism. It is not within ace here to criticise any of lusions, but they have ex- a very significant way the entertained by him that ıich are not justified by and possibly may not be justification (those for instance of immortality and the existence of the Deity), may nevertheless not only continue to exist in cultivated minds, but may also be profitably encouraged there, at any rate in the shape of hopes, for certain supposed advantages attendant on their retention, irrespective even of their truth.

express understanding that we do not guarantee its accuracy. Our belief in some single events, for example, might be correct, and yet that in a compound of several (if derived merely from our instinctive laws of belief) very possibly might not be correct, but might lead us into practical mistakes if we determined to act upon it. Even if the two were in accordance, this accordance would have to be proved, which would lead us round, by what I cannot but think a circuitous process, to the point which has been already chosen for commencing with.

§ 12. De Morgan seems to imply that the doctrine criticised above finds a justification from the analogy of Formal Logic. If the laws of necessary inference can be studied apart from all reference to external facts (except by way of illustration), why not those of probable inference? There does not, however, seem to be much force in any such analogy. Formal Logic, at any rate under its modern or Kantian mode of treatment, is based upon the assumption that there are laws of thought as distinguished from laws of things, and that these laws of thought can be ascertained and studied without taking into account their reference to any particular object. Now so long as we are confined to necessary or irreversible laws, as is of course the case in ordinary Formal Logic, this assumption leads to no special difficulties. We mean by this, that no conflict arises between these subjective and objective necessities. The two exist in perfect harmony side by side, the one being the accurate counterpart of the other. So precise is the correspondence between them, that few persons would notice, until study of metaphysics had called their attention to such points, that there were these two sides to the question. They would make their appeal to either with equal confidence, saying indifferently, 'the thing must be so,' or, 'we cannot conceive its being ərwise.' In fact it is only since the time of Kant that mental analysis has been to any extent appreciated and ɘpted. And even now the dominant experience school of losophy would not admit that there are here two really inct sides to the phenomenon; they maintain either that subjective necessity is nothing more than the consence by inveterate association of the objective uniformity, ɘlse that this so-called necessity (say in the Law of Conliction) is after all merely verbal, merely a different way of ng the same thing over again in other words. Whatever explanation adopted, the general result is that fallacies, eal acts of thought, are impossible within the domain of e logic; error within that province is only possible by a nentary lapse of attention, that is of consciousness.

§ 13. But though this perfect harmony between subive and objective uniformities or laws may exist within domain of pure logic, it is far from existing within that probability. The moment we make the *quantity* of our ef an integral part of the subject to be studied, any such uriable correspondence ceases to exist. In the former , we could not consciously think erroneously even though might try to do so; in the latter, we not only can believe neously but constantly do so. Far from the quantity of belief being so exactly adjusted in conformity with the s to which it refers that we cannot even in imagination ıstray, we find that it frequently exists in excess or defect hat which subsequent judgment will approve. Our incts of credence are unquestionably in frequent hostility ı experience; and what do we do then? We simply lify the instincts into accordance with the things. We constantly performing this practice, and no cultivated d would find it possible to do anything else. No man ld think of divorcing his belief from the things on which it was exercised, or would suppose that the former had an thing else to do than to follow the lead of the latter. Hen it results that that separation of the subjective necessity fro the objective, and that determination to treat the form as a science apart by itself, for which a plausible defen could be made in the case of pure logic, is entirely ina missible in the case of probability. However we mig contrive to '*think*' aright without appeal to facts, we ca not *believe* aright without incessantly checking our pr ceedings by such appeals. Whatever then may be t claims of Formal Logic to rank as a separate science, does not appear that it can furnish any support to th theory of Probability at present under examination.

§ 14. The point in question is sometimes urged a follows. Suppose a man with two, and only two, altern tives before him, one of which he knows must invol success and the other failure. He knows nothing mo about them than this, and he is forced to act. Would h not regard them with absolutely similar and equal feelin of confidence, without the necessity of referring them to an real or imaginary series? If so, is not this equivalent t saying that his belief of either, since one of them mus come to pass, is equal to that of the other, and therefore tha his belief of each is one-half of full confidence? Similarly if there are more than two alternatives: let it be supposed that there are any number of them, amongst which n distinctions whatever can be discerned except in such particulars as we know for certain will not affect the result; should we not feel equally confident in respect of each o them? and so here again should we not have a fractional estimate of our absolute amount of belief? It is thus attempted to lay the basis of a pure science of Probability, determining the distribution and combination of our belief hypothetically; viz. *if* the contingencies are exactly alike, then our belief is so apportioned, the question whether the contingencies are equal being of course decided as the objective data of Logic or Mathematics are decided.

To discuss this question fully would require a statement at some length of the reasons in favour of the objective or material view of Logic, as opposed to the Formal or Conceptualist. I shall have to speak on this subject in another chapter, and will not therefore enter upon it here. But one conclusive objection which is applicable more peculiarly to Probability may be offered at once. To pursue the line of enquiry just indicated, is, as already remarked, to desert the strictly logical ground, and to take up that appropriate to psychology; the proper question, in all these cases, being not what *do* men believe, but what ought they to believe? Admitting, as was done above, that in the case of Formal Logic these two enquiries, or rather those corresponding to them, practically run into one, owing to the fact that men cannot consciously 'think' wrongly; it cannot be too strongly insisted on that in Probability the two are perfectly separable and distinct. It is of no use saying what men do or will believe, we want to know what they will be right in believing; and this can never be settled without an appeal to the phenomena themselves.

§ 15. But apart from the above considerations, this way of putting the case does not seem to me at all conclusive. Take the following example. A man[1] finds himself on the sands of the Wash or Morecambe Bay, in a dense mist, when the spring-tide is coming in; and knows therefore that to be once caught by the tide would be fatal. He hears a church-bell at a distance, but has no means of knowing whether it is on the same side of the water with himself or on the opposite side. He cannot tell therefore whether by following its sound he will be led out into the mid-stream and be lost, or led back to dry land and safety. Here there can be no repetition of the event, and the cases are indistinguishably alike, to him, in the only circumstances which can affect the issue: is not then his prospect of death, it will be said, necessarily equal to one-half? A proper analysis of his state of mind would be a psychological rather than a logical enquiry, and in any case, as above remarked, the decision of this question does not touch our logical position. But according to the best introspection I can give I should say that what really passes through the mind in such a case is something of this kind: In most doubtful positions and circumstances we are accustomed to decide our conduct by a consideration of the relative advantages and disadvantages of each side, that is by the observed or inferred frequency with which one or the other alternative has succeeded. In proportion as these become more nearly balanced, we are more frequently mistaken in the individual cases; that is, it becomes more and more nearly what would be called 'a mere toss up' whether we are right or wrong. The case in question seems merely the limiting case, in which it has

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[1] It is necessary to take an example in which the man is forced to act, or we should not be able to shew that he has any belief on the subject at all. He may declare that he neither knows nor cares anything about the matter, and that therefore there is nothing of the nature of belief to be extracted out of his mental condition. He very likely would take this ground if we asked him, as De Morgan does, with a slightly different reference (*Formal Logic*, p. 183), whether he considers

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that there are volcanoes on the unseen side of the moon larger than those on the side turned towards us; or, with Jevons (*Principles of Science*, Ed. II. p. 212) whether he considers that a Platythliptic Coefficient is positive. These do not therefore seem good instances to illustrate the position that we always entertain a certain degree of belief on every question which can be stated, and that utter inability to give a reason in favour of either alternative corresponds to half belief.

ontrived that there shall be no appreciable difference
en the alternatives, by which to decide in favour of
other, and we accordingly feel no confidence in the
ilar result. Having to decide, however, we decide ac-
g to the precedent of similar cases which have occurred
To stand still and wait for better information is
death, and we therefore appeal to and employ the
ule we know of; or rather we feel, or endeavour to
we have felt before when acting in the presence of
atives as nearly balanced as possible. But I can
r perceive in my own case, nor feel convinced in that
ers, that this appeal, in a case which cannot be re-
[1], to a rule acted on and justified in cases which can be
e repeated, at all forces us to admit that our state of
s the same in each case.

6. This example serves to bring out very clearly a point
has been already mentioned, and which will have to be
d upon again, viz. that all which Probability discusses
statistical frequency of events, or, if we prefer so to
the quantity of belief with which any one of these
should be individually regarded, but leaves all the
uent conduct dependent upon that frequency, or that
to the choice of the agents. Suppose there are two
ers in the predicament in question: shall they keep
er, or separate in opposite directions? In either case
he chance of safety to each is the same, viz. one-half,
arly their circumstances must decide which course it
erable to adopt. If they are husband and wife, they will
ly prefer to remain together; if they are sole deposi-
f an important state secret, they may decide to part.
er words, we have to select here between the two alter-
of the certainty of a single loss, and the even chance

xcept indeed on the principles indicated further on in §§ 24, 25.

of a double loss; alternatives which the common ma
matical statement of their chances has a decided tend
to make us regard as indistinguishable from one anot
But clearly the decision must be grounded on the de
feelings, and conscience of the agents. Probability ca
say a word upon this question. As I have pointed out e
where, there has been much confusion on this matte
applications of the science to betting, and in the discus
of the Petersburg problem.

We have thus examined the doctrine in question w
a minuteness which may seem tedious, but in consequen
the eminence of its supporters it would have been pres
tuous to have rejected it without the strongest grounds.
objections which have been urged might be summaris
follows:—the amount of our belief of any given proposi
supposing it to be in its nature capable of accurate dete
nation (which does not seem to be the case), depends up
great variety of causes, of which statistical frequency—
subject of Probability—is but one. That even if we co
our attention to this one cause, the natural amount of
belief is not necessarily what theory would assign, but ha
be checked by appeal to experience. The subjective sid
Probability therefore, though very interesting and well
serving of examination, seems a mere appendage of the ob
tive, and affords in itself no safe ground for a scien
inference.

§ 17. The conception then of the science of Probab
as a science of the laws of belief seems to break dow
every point. We must not however rest content with
merely negative criticism. The degree of belief we en
tain of a proposition may be hard to get at accurately,
when obtained may be often wrong, and may need ther
to be checked by an appeal to the objects of belief. Sti

ular estimation we do seem to be able with more or less
racy to form a graduated scale of intensity of belief.
at we have to examine now is whether this be possible,
, if so, what is the explanation of the fact?

That it is generally believed that we can form such a
e scarcely admits of doubt. There is a whole vocabulary
common expressions such as, 'I feel almost sure,' 'I do not
quite certain,' 'I am less confident of this than of that,'
d so on. When we make use of any one of these phrases
seldom doubt that we have a distinct meaning to convey
means of it. Nor do we feel much at a loss, under any
n circumstances, as to which of these expressions we
uld employ in preference to the others. If we were asked
rrange in order, according to the intensity of the belief
h which we respectively hold them, things broadly marked
from one another, we could do it from our consciousness
belief alone, without a fresh appeal to the evidence upon
ich the belief depended. Passing over the looser proposi-
ns which are used in common conversation, let us take but
e simple example from amongst those which furnish nume-
l data. Do I not feel more certain that some one will die
s week in the whole town, than in the particular street in
ich I live? and if the town is known to contain a popula-
n one hundred times greater than that in the street, would
t almost any one be prepared to assert on reflection that
felt a hundred times more sure of the first proposition
n of the second? Or to take a non-numerical example, are
not often able to say unhesitatingly which of two propo-
ons we believe the most, and to some rough degree how
h more we believe one than the other, at a time when all
evidence upon which each rests has faded from the
l, so that each has to be judged, as we may say, solely on
own merits?

Here then a problem proposes itself. If popular o as illustrated in common language, be correct,—an considerable weight must of course be attributed to it,- does exist something which we call partial belief in ence to any proposition of the numerical kind de above. Now what we want to do is to find some justification of this belief, to obtain in fact some intel answer to the question, Is it correct? We shall find in ally that the answer to this question will throw a goo of light upon another question nearly as important a more intricate, viz. What is the meaning of this partial

§ 18. ~~We shall~~ find it advisable ~~to commence~~ by ~~taining how such~~ enquiries as ~~the above would~~ be an in the ~~case~~ of ordinary full belief. Such a step wou offer the slightest difficulty. Suppose, to take a example, that we have obtained the following proposi whether by induction, or by the rules of ordinary de logic, does not matter for our present purpose,—that a mixture of oxygen and hydrogen is explosive. Here w an inference, and consequent belief of a proposition. suppose there were any enquiry as to whether our were correct, what should we do? The simplest settling ~~the~~ matter would be to ~~find out by a~~ distinct to experience whether the ~~proposition~~ was true. Si ~~are~~ reasoning about things, the justification of the beli is, ~~the test of its correctness,~~ would be most readily fo the truth of the proposition. If by any process of in I have come to believe that a certain mixture will exp consider my belief to be justified, that is to be con under proper circumstances the explosion always does if it does not occur the belief was wrong.

Such an answer, no doubt, goes but a little way, or no way at all, towards explaining what is the nature of

~~tself; but~~ it is sufficient for our present purpose, which is ely that of determining ~~what is meant by the correctness~~ ~~ur belief,~~ and by the test of its correctness. In all infers about things, in which the *amount* of our belief is not n into account, such an explanation as the above is quite cient; it would be the ordinary one in any question of nce. It is moreover perfectly intelligible, whether the lusion is particular or universal. Whether we believe t 'some men die', or that 'all men die', our ~~belief~~ may with ~~al ease~~ be tested by the ~~appropriate~~ train of experience.

§ 19. But when we attempt to apply the same test to *ial* belief, we shall find ourselves reduced to an awkward lexity. A difficulty now emerges which has been singuy overlooked by those who have treated of the subject. simple example will serve our purpose, we will take the of a penny. I am about to toss one up, and I therefore believe, to adopt the current language, that it will give d. Now it seems to be overlooked that if we appeal to event, as we did in the case last examined, our belief st inevitably be wrong, and therefore the test above mened will fail. For the thing must either happen or not pen: i.e. in this case the penny must either give head, or give it; there is no third alternative. But whichever y it occurs, our half-belief, so far as such a state of mind mits of interpretation, must be wrong. If head does come, n wrong in not having expected it enough; for I only half ieved in its occurrence. If it does not happen, I am ally wrong in having expected it too much; for I half ieved in its occurrence, when in fact it did not occur at all.

The same difficulty will occur in every case in which we tempt to justify our state of partial belief in a single congent event. Let us take another example, slightly differfrom the last. A man is to receive £1 if a die gives six,

to pay 1*s*. if it gives any other number. It will generall
admitted that he ought to give 2*s*. 6*d*. for the chance,
that if he does so he will be paying a fair sum. Thi
ample only differs from the last in the fact that inste
simple belief in a proposition, we have taken what math
ticians call 'the *value* of the expectation'. In other w
we have brought into greater prominence, not merely th
lief, but the conduct which is founded upon the belief.
precisely the same difficulty recurs here. For appealing to
event,—the single event, that is,—we see that one or
party must lose his money without compensation. In
sense then can such an expectation be said to be a fair

§ 20. A possible answer to this, and so far as appear
only possible answer, will be, that what we really mean
saying that we half believe in the occurrence of head i
express our conviction that head will certainly happen
the average every other time. And similarly, in the se
example, by calling the sum a fair one it is meant tha
the long run neither party will gain or lose. As we
recur presently to the point raised in this form of answer,
only notice that need be taken of it at this point is to
attention to the fact that it entirely abandons the w
question in dispute, for it admits that this partial belief
not in any strict sense apply to the individual event, sinc
clearly cannot be justified there. At such a result indeed
cannot be surprised; at least we cannot on the theory adop
throughout this Essay. For bearing in mind that the
ployment of Probability postulates ignorance of the si
event, it is not easy to see how we are to justify any
opinion or statement about the single event than a co
sion of such ignorance.

§ 21. So far then we do not seem to have made
slightest approximation to a solution of the partic

stion now under examination. The more closely we have lysed special examples, the more unmistakeably are we ught to the conclusion that in the individual instance no tification of anything like quantitative belief is to be nd; at least none is to be found in the same sense in ich we expect it in ordinary scientific conclusions, whether luctive or Deductive. And yet we have to face and ount for the fact that common impressions, as attested by whole vocabulary of common phrases, are in favour of the stence of this quantitative belief. How are we to account this? If we appeal to an example again, and analyse it ewhat more closely, we may yet find our way to some isfactory explanation.

In our previous analysis (§ 18) we found it sufficient to stop an early stage, and to give as the justification of our belief e fact of the proposition being true. Stopping however at t stage, we have found this explanation fail altogether to ve a justification of partial belief; fail, that is, when applied the individual instance. The two states of belief and dis- lief correspond admirably to the two results of the event ppening and not happening respectively, and unless for ychological purposes we saw no reason to analyse further; t to partial belief there is nothing corresponding in the re- lt, for the event cannot partially happen in such cases as we e concerned with. Suppose then we advance a step further the analysis, and ask again what is meant by the proposi- n being true? This introduces us, of course, to a very long d intricate path; but in the short distance along it which e shall advance, we shall not, it is to be hoped, find any ry serious difficulty. As before, we will illustrate the alysis by first applying it to the case of ordinary full belief.

§ 22. Whatever opinion then may be held about the ential nature of belief, it will probably be admitted that a

readiness to act upon the proposition believed is an insep
able accompaniment of that state of mind. There can b
alteration in our belief (at any rate in the case of sane pers
without a possible alteration in our conduct, nor anythin
our conduct which is not connected with something in
belief. We will first take an example in connection with
penny, in which there is full belief; we will analyse it a
further than we did before, and then attempt to apply
same analysis to an example of a similar kind, but on
which the belief is partial instead of full.

Suppose that I am about to throw a penny up, and c
template the prospect of its falling upon one of its sides
not upon its edge. We feel perfectly confident that it
do so. Now whatever else may be implied in our belief,
certainly mean this; that we are ready to stake our cond
upon its falling thus. All our betting, and everything
that we do, is carried on upon this supposition. Any
whatever that might ensue upon its falling otherwise will
incurred without fear. This, it must be observed, is equ
the case whether we are speaking of a single throw or
long succession of throws.

But now let us take the case of a penny falling, not up
one side or the other, but upon a given side, *head.* To
certain extent this example resembles the last. We are p
fectly ready to stake our conduct upon what comes to pass
the long run. When we are considering the result of a la
number of throws, we are ready to act upon the suppositi
that head comes every other time. If e.g. we are betti
upon it, we shall not object to paying £1 every time th
head comes, on condition of receiving £1 every time th
head does not come. This is nothing else than the *transl*
tion, as we may call it, into practice, of our belief that he
and tail occur equally often.

Now it will be obvious, on a moment's consideration, that ur conduct is capable of being slightly varied: of being ried, that is, in form, whilst it remains identical in respect its results. It is clear that to pay £1 every time we lose, d to get £1 every time we gain, comes to precisely the same ing, in the case under consideration, as to pay ten shillings ery time without exception, and to receive £1 every time at head occurs. It is so, because heads occur, on the erage, every other time. In the long run the two results oincide; but there is a marked difference between the two ses, considered individually. The difference is two-fold. the first place we depart from the notion of a payment ery other time, and come to that of one made every time. the second place, what we pay every time is half of what e get in the cases in which we do get anything. The difrence may seem slight; but mark the effect when our conct is translated back again into the subjective condition pon which it depends, viz. into our belief. It is in conseuence of such a translation, as it appears to me, that the otion has been acquired that we have an accurately determinable amount of belief as to every such proposition. To ve losses and gains of equal amount, and to incur them qually often, was the experience connected with our belief at the two events, head and tail, would occur equally often. his was quite intelligible, for it referred to the long run. o find that this could be commuted for a payment made very time without exception, a payment, observe, of half the mount of what we occasionally receive, has very naturally en interpreted to mean that there must be a state of halfelief which refers to each individual throw.

§ 23. One such example, of course, does not go far toards establishing a theory. But the reader will bear in mind that almost all our conduct tends towards the same result; that it is not in betting only, but in every cours action in which we have to count the events, that suc numerical apportionment of our conduct is possible. He by the ordinary principles of association, it would ap exceedingly likely that, not exactly a numerical conditio mind, but rather numerical associations, become insepara connected with each particular event which we know to oc in a certain proportion of times. Once in six times a gives ace; a knowledge of this fact, taken in combina with all the practical results to which it leads, produces, cannot doubt, an inseparable notion of one-sixth connec with each *single* throw. But it surely cannot be called be to the amount of one-sixth; at least it admits neithe justification nor explanation in these single cases, to wh alone the fractional belief, if such existed, ought to apply.

It is in consequence, I apprehend, of such association t we act in such an unhesitating manner in reference to single contingent event, even when we have no expecta of its being repeated. A die is going to be thrown up o and once only. I bet 5 to 1 against ace, not, as is commo asserted, because I feel one-sixth part of certainty in occurrence of ace; but because I know that such cond would be justified in the long run of such cases, and I ap to the solitary individual the same rule that I should ap to it if I knew it were one of a long series. This accou for my conduct being the same in the two cases; by asso tion, moreover, we probably experience very similar feeli in regard to them both.

§ 24. And here, on the view of the subject adopted this Essay, we might stop. We are bound to explain 'measure of our belief' in the occurrence of a single ev when we judge solely from the statistical frequency w which such events occur, for such a series of events was tarting-point; but we are not bound to inquire whether in very case in which persons have, or claim to have, a certain measure of belief there must be such a series to which to efer it, and by which to justify it. Those who start from he subjective side, and regard Probability as the science of uantitative belief, are obliged to do this, but we are free rom the obligation.

Still the question is one which is so naturally raised in onnection with this subject, that it cannot be altogether assed by. I think that to a considerable extent such a ustification as that mentioned above will be found applicable o other cases. The fact is that we are very seldom called upon to decide and act upon a single contingency which cannot be viewed as being one of a series. Experience introduces us, it must be remembered, not merely to a succession of events neatly arranged in a single series (as we have hitherto assumed them to be for the purpose of illustration), but to an infinite number belonging to a vast variety of different series. A man is obliged to be acting, and therefore exercising his belief about one thing or another, almost the whole of every day of his life. Any one person will have to decide in his time about a multitude of events, each one of which may never recur again within his own experience. But by the very fact of there being a multitude, though they are all of different kinds, we shall still find that order is maintained, and so a course of conduct can be justified. In a plantation of trees we should find that there is order of a certain kind if we measure them in any one direction, the trees being on an average about the same distance from each other. But a somewhat similar order would be found if we were to examine them in any other direction whatsoever. So in nature generally; there is regularity in a succession of events of the same kind. But there may also be regularity if we form a series by taking successively a number out totally distinct kinds.

It is in this circumstance that we find an extension of th practical justification of the measure of our belief. A ma say, buys a life annuity, insures his life on a railway journ puts into a lottery, and so on. Now we may make a seri out of these acts of his, though each is in itself a single eve which he may never intend to repeat. His conduct, and therefore his belief, measured by the result in each individu instance, will not be justified, but the reverse, as shewn § 19. Could he indeed repeat each kind of action oft enough it would be justified; but from this, by the conditio of life, he is debarred. Now it is perfectly conceivable th in the new series, formed by his successive acts of differe kinds, there should be no regularity. As a matter of fac however, it is found that there is regularity. In this way t equalization of his gains and losses, for which he cannot ho in annuities, insurances, and lotteries taken separately, ma yet be secured to him out of these events taken collectivel If in each case he values his chance at its right proportio (and acts accordingly) he will in the course of his life neith gain nor lose. And in the same way if, whenever he has th alternative of different courses of conduct, he acts in accordance with the estimate of his belief described above, i. chooses the event whose chance is the best, he will in the en gain more in this way than by any other course. By the existence, therefore, of these *cross-series*, as we may term the there is an immense addition to the number of actions whic may be fairly considered to belong to those courses of conduct which offer many successive opportunities of equalizin gains and losses. All these cases then may be regarded a admitting of justification in the way now under discussion.

§ 25. In the above remarks it will be observed that w ave been giving what is to be regarded as a justification of is belief from the point of view of the individual agent him- lf. If we suppose the existence of an enlarged fellow-feel- g, the applicability of such a justification becomes still more tensive. We can assign a very intelligible sense to the sertion that it is 999 to 1 that I shall not get a prize in a ttery, even if this be stated in the form that my belief in y so doing is represented by the fraction $\frac{1}{1000}$th of certainty. operly it means that in a very large number of throws I ould gain once in 1000 times. If we include other contin- ncies of the same kind, as described in the last section, ch individual may be supposed to reach to something like is experience within the limits of his own life. He could t do it in this particular line of conduct alone, but he could it in this line combined with others. Now introduce the ssibility of each man feeling that the gain of others offers me analogy to his own gains, which we may conceive his ing except in the case of the gains of those against whom is directly competing, and the above justification becomes ll more extensively applicable.

The following would be a fair illustration to test this ew. I know that I must die on some day of the week, and ere are but seven days. My belief, therefore, that I shall e on a Sunday is one-seventh. Here the contingent event clearly one that does not admit of repetition; and yet ould not the belief of every man have the value assigned it the formula? It would appear that the same principle ll be found to be at work here as in the former examples. is quite true that I have only the opportunity of dying ce myself, but I am a member of a class in which deaths cur with frequency, and I form my opinion upon evidence awn from that class. If, for example, I had insured my e for £1000, I should feel a certain propriety in demanding

£7000 in case the office declared that it would only pa the event of my dying on a Sunday. *I*, indeed, for my private part, might not find the arrangement an equit one; but mankind at large, in case they acted on su principle, might fairly commute their aggregate gains in a way, whilst to the Insurance Office it would not make difference at all.

§ 26. The results of the last few sections might be s marised as follows:—the different amounts of belief w we entertain upon different events, and which are recogn by various phrases in common use, have undoubtedly s meaning. But the greater part of their meaning, and tainly their only justification, are to be sought in the s of corresponding events to which they belong; in regar which it may be shewn that far more events are capabl being referred to a series than might be supposed at sight. The test and justification of belief are to be foun conduct; in this test applied to the series as a whole, t is nothing peculiar, it differs in no way from the similar when we are acting on our belief about any single ev But so applied, from the nature of the case it is app successively to each of the individuals of the series; here *conduct* generally admits of being separately considere reference to each particular event; and this has been un stood to denote a certain amount of belief which should l fraction of certainty. Probably on the principles of asso tion, a peculiar condition of mind is produced in referenc each single event. And these associations are not unnatur retained even when we contemplate any one of these si events isolated from any series to which it belongs. W it is found alone we treat it, and feel towards it, as we when it is in company with the rest of the series.

§ 27. We may now see, more clearly than we c

ore, why it is that we are free from any necessity of as-
ning the existence of causation, in the sense of necessary
ariable sequence, in the case of the events which compose
series. Against such a view it might very plausibly be
ged, that we constantly talk of the probability of a single
nt; but how can this be done, it may reasonably be said,
we once admit the possibility of that event occurring for-
tously? Take an instance from human life; the average
ration of the lives of a batch of men aged thirty will be
ut thirty-four years. We say therefore to any individual
them, Your expectation of life is thirty-four years. But
can this be said if we admit that the train of events
nposing his life is liable to be destitute of all regular
uence of cause and effect? To this it may be replied
t the denial of causation enables us to say neither more
r less than its assertion, in reference to the length of the
ividual life, for of this we are ignorant in each case alike.
assigning, as above, an expectation in reference to the
ividual, we *mean* nothing more than to make a statement
out the average of his class. Whether there be causation
not in these individual cases does not affect our knowledge
the average, for this by supposition rests on independent
perience. The legitimate inferences are the same on either
pothesis, and of equal value. The only difference is that
the hypothesis of non-causation we have forced upon our
ention the impropriety of talking of the 'proper' expecta-
n of the individual, owing to the fact that all knowledge of
amount is formally impossible; on the other hypothesis
impropriety is overlooked from the fact of such know-
ge being only practically unattainable. As a matter of
t the amount of our knowledge is the same in each case;
is a knowledge of the average, and of that only[1].

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[1] For a fuller discussion of this, see the Chapter on Causation.

§ 28. We may conclude, then, that the limits wi which we are thus able to justify the amount of our b are far more extensive than might appear at first si Whether every case in which persons feel an amoun belief short of perfect confidence could be forced into province of Probability is a wider question. Even, howe if the belief could be supposed capable of justification or principles, its rules could never in such cases be made us Suppose, for example, that a father were in doubt whet to give a certain medicine to his sick child. On the hand, the doctor declared that the child would die unless medicine were given; on the other, through a mistake, father cannot feel quite sure that the medicine he has is right one. It is conceivable that some mathematicians, their conviction that everything has its definite numeri probability, would declare that the man's belief had so 'value' (if they could only find out what it is), say ni tenths; by which they would mean that in nine cases out ten in which he entertained a belief of that particular va he proved to be right. So with his belief and doubt the other side of the question. Putting the two togeth there is but one course which, as a prudent man and a g father, he can possibly follow. It may be so, but when here) the identification of an event in a series depends purely subjective conditions, as in this case upon the deg of vividness of his conviction, of which no one else can jud no test is possible, and therefore no proof can be found.

§ 29. So much then for the attempts, so frequen made, to found the science on a subjective basis; they c lead, as it has here been endeavoured to show, to no satisf tory result. Still our belief is so inseparably connected w our action, that something of a defence can be made for t attempts described above; but when it is attempted, as en the case, to import other sentiments besides pure belief, d to find a justification for them also in the results of our ence, the confusion becomes far worse. The following tract from Archbishop Thomson's *Laws of Thought* (§ 122, l. II.) will show what kind of applications of the science are ntemplated here: "In applying the doctrine of chances to at subject in connexion with which it was invented—games chance,—the principles of what has been happily termed oral arithmetic' must not be forgotten. Not only would be difficult for a gamester to find an antagonist on terms, to fortune and needs, precisely equal, but also it is im- ssible that with such an equality the advantage of a nsiderable gain should balance the harm of a serious loss. f two men,' says Buffon, 'were to determine to play for eir whole property, what would be the effect of this agree- ent? The one would only double his fortune, and the her reduce his to naught. What proportion is there be- een the loss and the gain? The same that there is between l and nothing. The gain of the one is but a moderate m,—the loss of the other is numerically infinite, and orally so great that the labour of his whole life may not rhaps suffice to restore his property.'"

As moral advice this is all very true and good. But if it regarded as a contribution to the science of the subject it quite inappropriate, and seems calculated to cause con- sion. The doctrine of chances pronounces upon certain nds of events in respect of number and magnitude; it has solutely nothing to do with any particular person's feelings out these relations. We might as well append a corollary the rules of arithmetic, to point out that although it is ry true that twice two are four it does not follow that four rses will give twice as much pleasure to the owner as two ill. If two men play on equal terms their chances are

equal; in other words, if they were often to play in
manner each would lose as frequently as he would gain. T
is all that Probability can say; what under the circumsta
may be the determination and opinions of the men in qu
tion, it is for them and them alone to decide. There
many persons who cannot bear mediocrity of any kind,
to whom the prospect of doubling their fortune would
weigh a greater chance of losing it altogether. They al
are the judges.

If we will introduce such a balance of pleasure and p
the individual must make the calculation for himself. T
supposition is that total ruin is very painful, partial l
painful in a less proportion than that assigned by the r
of the losses themselves; the inference is therefore dra
that on the average more pain is caused by occasional gr
losses than by frequent small ones, though the money va
of the losses in the long run may be the same in each c
But if we suppose a country where the desire of spendi
largely is very strong, and where owing to abundant produ
tion loss is easily replaced, the calculation might incline t
other way. Under such circumstances it is quite possi
that more happiness might result from playing for high th
for low stakes. The fact is that all emotional consideratio
of this kind are irrelevant; they are, at most, mere appli
tions of the theory, and such as each individual is alo
competent to make for himself. Some more remarks will
made upon this subject in the chapter upon Insurance a
Gambling.

§ 30. It is by the introduction of such considerations
these that the Petersburg Problem has been so perplex
Having already given some description of this problem
will refer to it very briefly here. It presents us with
sequence of sets of throws for each of which sets I am

ve something, say a shilling, as the minimum receipt. receipts increase in proportion to the rarity of each cular kind of set, and each kind is observed or inferred ow more rare in a certain definite but unlimited order. he wording of the problem, properly interpreted, I am osed never to stop. Clearly therefore, however large a pay for each of these sets, I shall be sure to make it up me. The mathematical expression of this is, that I t always to pay an infinite sum. To this the objection posed, that no sensible man would think of advancing a large finite sum, say £50. Certainly he would not; why? Because neither he nor those who are to pay would be likely to live long enough for him to obtain ws good enough to remunerate him for one-tenth of his y; to say nothing of his trouble and loss of time. We not suppose that the problem, as stated in the ideal , will coincide with the practical form in which it nts itself in life. A carpenter might as well object to id's second postulate, because his plane came to a stop x feet on the plank on which he was at work. Many ons have failed to perceive this, and have assumed that, les enabling us to draw numerical inferences about the bers of a series, the theory ought also to be called upon stify all the opinions which average respectable men t be inclined to form about them, as well as the conduct might choose to pursue in consequence. It is obvious to enter upon such considerations as these is to diverge our proper ground. We are concerned, in these cases, the actions of men only, as given in statistics; with the ions they experience in the performance of these actions ave no direct concern whatever. The error is the same any one were to confound, in political economy, value in with value in exchange, and object to measuring the value of a loaf by its cost of production, because bre worth more to a man when he is hungry than it is just his dinner.

§ 31. One class of emotions indeed ought to be cepted, which, from the apparent uniformity and co ency with which they show themselves in different pe and at different times, do really present some better cla consideration. In connection with a science of infe they can never indeed be regarded as more than an acc of what is essential to the subject, but compared with emotions they seem to be inseparable accidents.

The reader will remember that attention was draw the earlier part of this chapter to the compound natu the state of mind which we term belief. It is partly lectual, partly also emotional; it professes to rest experience, but in reality the experience acts throug distorting media of hopes and fears and other distu agencies. So long as we confine our attention to the *of mind* of the person who believes, it appears to me these two parts of belief are quite inseparable. Inde speak of them as two parts may convey a wrong impres for though they spring from different sources, they s tirely merge in one result as to produce what migh called an indistinguishable compound. Every kind of ence, whether in probability or not, is liable to be dist in this way. A timid man may honestly believe that h be wounded in a coming battle, when others, with the experience but calmer judgments, see that the chan too small to deserve consideration. But such a man's if we look only to that, will not differ in its nature sound belief. His conduct also in consequence of his will by itself afford no ground of discrimination; he make his will as sincerely as a man who is unmistakeab death-bed. The only resource is to check and correct belief by appealing to past and current experience[1]. This s advanced as an objection to the theory on which proba-ity is regarded as concerned primarily with laws of belief. t on the view taken in this Essay in which we are sup-ed to be concerned with laws of inference about things, r and difficulty from this source vanish. Let us bear clearly mind that we are concerned with inferences about things, whatever there may be in belief which does not depend experience will disappear from notice.

§ 32. These emotions then can claim no notice as an egral portion of any science of inference, and should in ctness be rigidly excluded from it. But if any of them uniform and regular in their production and magnitude, y may be fairly admitted as accidental and extraneous mpaniments. This is really the case to some extent h our surprise. This emotion does show a considerable ree of uniformity. The rarer any event is the more am I, common with most other men, surprised at it when it does ppen. This surprise may range through all degrees, from most languid form of interest up to the condition which term 'being startled'. And since the surprise seems be pretty much the same, under similar circumstances, different times, and in the case of different persons, it is e from that extreme irregularity which is found in most the other mental conditions which accompany the contemplation of unexpected events. Hence our surprise, th as stated above, having no proper claim to admission the science of Probability, is such a constant and re accompaniment of that which Probability is concerned that notice must often be taken of it. References wi casionally be found to this aspect of the question i following chapters.

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[1] The best example I can recall the distinction between judging the subjective and the objec- side, in such cases as these, urred once in a railway train. et a timid old lady who was much fear of accidents. I en-voured to soothe her on the usual statistical ground of the extreme rarity of such events. She listened patiently, and then replied, "Yes, Sir, that is all very well; but I don't see how the real danger will be a bit the less because I don't believe in it."

It may be remarked in passing, for the sake of fu illustration of the subject, that this emotional accom ment of surprise, to which we are thus able to assign thing like a fractional value, differs in two important res from the commonly accepted fraction of belief. In the place, it has what may be termed an independent exist it is intelligible by itself. The belief, as we endeavour show, needs explanation and finds it in our consequent duct. Not so with the emotion; this stands upon its footing, and may be examined in and by itself. Hen the second place, it is as applicable, and as capable of any of justification, in relation to the *single event*, as to a ser events. In this respect, as will be remembered, it off complete contrast to our state of belief about any one tingent event. May not these considerations help to ac for the general acceptance of the doctrine, that we h certain definite and measurable amount of belief about events? I cannot help thinking that what is so obvi true of the emotional portion of the belief, has been u sciously transferred to the other or intellectual portion o compound condition, to which it is not applicable, and w it cannot find a justification.

§ 33. A further illustration may now be given of subjective view of Probability at present under di sion.

An appeal to common language is always of servic

ıployment of any distinct word is generally a proof
ankind have observed some distinct properties in the
which have caused them to be singled out and have
ame appropriated to them. There is such a class of
assigned by popular usage to the kind of events of
Probability takes account. If we examine them we
nd, I think, that they direct us unmistakeably to the
d aspect of the question,—the objective and the sub-
, the quality in the events and the state of our minds
sidering them,—that have occupied our attention
the former chapters.

e word 'extraordinary', for instance, seems to point to
served fact, that events are arranged in a sort of *ordo*
c. No one of them might be so exactly placed that
ld have inferred its position, but when we take a great
into account together, running our eye, as it were,
the line, we begin to see that they really do for the
art stand in order. Those which stand away from the
ave this divergence observed, and are called ex-
inary, the rest ordinary, or in the line. So too 'irre-
and 'abnormal' are doubtless used from the appear-
f things, when examined in large numbers, being that
arrangement by rule or measure. This only holds
there are a good many; we could not speak of the
events being so arranged. Again the word 'law', in
ilosophical sense, has now become quite popularised.
the term became introduced is not certain, but
can be little doubt that it was somewhat in this
-The effect of a law, in its usual application to
n conduct, is to produce regularity where it did not
usly exist; when then a regularity began to be per-
in nature, the same word was used, whether the cause
upposed to be the same or not. In each case there

was the same generality of agreement, subject to occa deflection[1].

On the other hand, observe the words 'wonderful' expected', 'incredible'. Their connotation describes of mind simply; they are of course not confined to P bility, in the sense of statistical frequency, but imply s that the events they denote are such as from some cau did not expect would happen, and at which therefore, they do happen, we are surprised.

Now when we bear in mind that these two clas words are in their origin perfectly distinct;—the on noting simply events of a certain character; the though also denoting events, *connoting* simply stat mind;—and yet that they are universally applied t same events, so as to be used as perfectly synonymou have in this a striking illustration of the two sides which Probability may be viewed, and of the universal nition of a close connection between them. The wor popularly used as synonymous, and we must not press meaning too far; but if it were to be observed, as rather inclined to think it could, that the application words which denote mental states is wider than that others, we should have an illustration of what has already observed, viz. that the province of Probability so extensive as that over which variation of belief mi observed. Probability only considers the case in whic variation is brought about in a certain definite stat way.

§ 34. It will be found in the end both interestin important to have devoted some attention to this sub of the question. In the first place, as a mere specu- ve inquiry the quantity of our belief of any proposition erves notice. To study it at all deeply would be to tres- s into the province of Psychology, but it is so intimately nected with our own subject that we cannot avoid all rence to it. We therefore discuss the laws under which expectation and surprise at isolated events increases or inishes, so as to account for these states of mind in any ividual instance, and, if necessary, to correct them when y vary from their proper amount.

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[1] This would still hold of *empirical* laws which may be capable of being broken: we now have very much shifted the word, to denote *mate* law which it is supposed be broken.

But there is another more important reason than this. is quite true that when the subjects of our discussion in particular instance lie entirely within the province Probability, they may be treated without any reference ur belief. We may or we may not employ this side of question according to our pleasure. If, for example, I asked whether it is more likely that A. B. will die this , than that it will rain to-morrow, I may calculate the nce (which really is at bottom the same thing as my ief) of each, find them respectively, one-sixth and one- enth, say, and therefore decide that my 'expectation' of former is the greater, viz. that this is the more likely nt. In this case the process is precisely the same whether suppose our belief to be introduced or not; our mental te is, in fact, quite immaterial to the question. But, in er cases, it may be different. Suppose that we are com- ing two things, of which one is wholly alien to Proba- ity, in the sense that it is hopeless to attempt to assign degree of numerical frequency to it, the only ground y have in common may be the amount of belief to which y are respectively entitled. We cannot compare the quency of their occurrence, for one may occur too seldom judge by, perhaps it may be unique. It has been already said, that our belief of many events rests upon a very plicated and extensive basis. My belief may be the pro of many conflicting arguments, and many analogies mo less remote; these proofs themselves may have mostly f from my mind, but they will leave their effect behind t in a weak or strong conviction. At the time, therefo may still be able to say, with some degree of accu though a very slight degree, what amount of belief I en tain upon the subject. Now we cannot compare things are heterogeneous: if, therefore, we are to decide bet this and an event determined naturally and properl Probability, it is impossible to appeal to chances or frequ of occurrence. The measure of belief is the only com ground, and we must therefore compare this quantity in case. The test afforded will be an exceedingly rough for the reasons mentioned above, but it will be better none; in some cases it will be found to furnish all we w

Suppose, for example, that one letter in a million is in the Post Office, and that in any given instance I wis know which is more likely, that a letter has been so los that my servant has stolen it? If the latter altern could, like the former, be stated in a numerical form, comparison would be simple. But it cannot be reduce this form, at least not consciously and directly. Still, i could feel that our belief in the man's dishonesty was gre than one-millionth, we should then have homogeneous th before us, and therefore comparison would be possible.

§ 35. We are now in a position to give a tolerably a rate definition of a phrase which we have frequently obliged to employ, or incidentally to suggest, and of w the reader may have looked for a definition already, viz probability of an event, or what is equivalent to this, chance of any given event happening. I consider that t

presuppose a series; within the indefinitely numerous
which composes this series a smaller class is distin-
d by the presence or absence of some attribute or
ites, as was fully illustrated and explained in a pre-
chapter. These larger and smaller classes respectively
mmonly spoken of as instances of the 'event,' and of
appening in a given particular way.' Adopting this
ology, which with proper explanations is suitable
h, we may define the probability or chance (the terms
re regarded as synonymous) of the event happening
t particular way as the numerical fraction which repre-
the proportion between the two different classes in the
run. Thus, for example, let the probability be that
given infant living to be eighty years of age. The
series will comprise all infants, the smaller all who live
hty. Let the proportion of the former to the latter be
; in other words, suppose that one infant in ten lives
hty. Then the chance or probability that any given
will live to eighty is the numerical fraction $\frac{1}{10}$. This
es that the series are of indefinite extent, and of the
which we have described as possessing a fixed type.
s be not the case, but the series be supposed termi-
, or regularly or irregularly fluctuating, as might be the
for instance, in a society where owing to sanitary or
causes the average longevity was steadily undergoing
nge, then in so far as this is the case the series ceases
a subject of science. What we have to do under these
nstances, is to substitute a series of the right kind for
appropriate one presented by nature, choosing it, of
, with as little deflection as possible from the observed
This is nothing more than has to be done, and in-
ly is done, whenever natural objects are made subjects
ct science.

§ 36. A word or two of explanation may be added a the expression employed above, 'the proportion in the run.' The run must be supposed to be very long indee fact never to stop. As we keep on taking more terms of series we shall find the proportion still fluctuating a l but its fluctuations will grow less. The proportion, in will gradually approach towards some fixed numerical v what mathematicians term its *limit*. This fractional v is the one spoken of above. In the cases in which dedu reasoning is possible, this fraction may be obtained wit direct appeal to statistics, from reasoning about the ditions under which the events occur, as was explaine the fourth chapter.

Here becomes apparent the full importance of the tinction so frequently insisted on, between the actual irreg series before us and the substituted one of calculation, the meaning of the assertion (Ch. I. § 13), that it was in case of the latter only that strict scientific inferences c be made. For how can we have a 'limit' in the cas those series which ultimately exhibit irregular fluctuati When we say, for instance, that it is an even chance a given person recovers from the cholera, the meanin this assertion is that in the long run one half of the per attacked by that disease do recover. But if we exam a sufficiently extensive range of statistics, we might that the manners and customs of society had produced a change in the type of the disease or its treatment, tha were no nearer approaching towards a fixed limit tha were at first. The conception of an ultimate limit in ratio between the numbers of the two classes in the s necessarily involves an absolute fixity of the type. W therefore nature does not present us with this absolute fi as she seldom or never does except in games of chance t demonstrably there), our only resource is to introduce ch a series, in other words, as has so often been said, to bstitute a series of the right kind.

§ 37. The above, which may be considered tolerably mplete as a definition, might equally well have been ven in the last chapter. It has been deferred however the present place, in order to connect with it at once a oposition involving the conceptions introduced in this apter; viz. the state of our own minds, in reference to the aount of belief we entertain in contemplating any one the events whose probability has just been described. easons were given against the opinion that our belief aditted of any exact apportionment like the numerical one st mentioned. Still, it was shown that a reasonable explation could be given of such an expression as, 'my belief is th of certainty', though it was an explanation which pointed mistakeably to a series of events, and ceased to be intelgible, or at any rate justifiable, when it was not viewed in ch a relation to a series. In so far, then, as this expla~~tion is adopted, we may~~ say that our belief is in proortion to the above fraction. This referred to the purely ~~tellectual part of belief which cannot be conceived~~ to be parable, even in thought, from the things upon which it ~~exercised.~~ With this intellectual part there are comnly associated various emotions. These we can to a rtain extent separate, and, when separated, can measure ith that degree of accuracy which is possible in the case of her emotions. They are moreover intelligible in reference the individual events. They will be found to increase d diminish in accordance, to some extent, with the fraction hich represents the scarcity of the event. The emotion of rprise does so with some degree of accuracy.

The above investigation describes, though in a very brief

form, the amount of truth which appears to me to be co
tained in the assertion frequently made, that the fracti
expressive of the probability represents also the fractio
part of full certainty to which our belief of the individu
event amounts. Any further analysis of the matter wo
seem to belong to Psychology rather than to Probability.

CHAPTER VII.

THE RULES OF INFERENCE IN PROBABILITY.

IN the previous chapter, an investigation was made into at may be called, from the analogy of Logic, Immediate erences. Given that nine men out of ten, of any assigned , live to forty, what could be inferred about the prospect life of any particular man? It was shown that, although is step was very far from being so simple as it is frequently pposed to be, and as the corresponding step really is in ogic, there was nevertheless an intelligible sense in which e might speak of the amount of our belief in any one of ese 'proportional propositions,' as they may succinctly be med. and justify that amount. We must now proceed to e consideration of inferences more properly so called, I ean inferences of the kind analogous to those which form the aple of ordinary logical treatises. In other words, having certained in what manner particular propositions could be ferred from the general propositions which included them, e must now examine in what cases one general proposition n be inferred from another. By a general proposition here meant, of course, a general proposition of the statistical nd contemplated in Probability. The rules of such infer- ce being very few and simple, their consideration will not tain us long. From the data now in our possession we are able to deduce the rules of probability given in ord treatises upon the science. It would be more correct t that we are able to deduce *some* of these rules, for, a appear on examination, they are of two very different l resting on entirely distinct grounds. They might be di into those which are formal, and those which are more o experimental. This may be otherwise expressed by s that, from the kind of series described in the first cha some rules will follow necessarily by the mere applicati arithmetic; whilst others either depend upon peculiar l theses, or demand for their establishment continuall newed appeals to experience, and extension by the aid c various resources of Induction. We shall confine our a tion at present principally to the former class; the latte only be fully understood when we have considered the nection of our science with Induction.

§ 2. The fundamental rules of Probability strict called, that is the formal rules, may be divided into classes,—those obtained by addition or subtraction o one hand, corresponding to what are generally terme connection of exclusive or incompatible events[1]; and obtained by multiplication or division, on the other corresponding to what are commonly termed depe events. We will examine these in order.

(1) We can make inferences by simple additior for instance, there are two distinct properties observa various members of the series, which properties do not in the same individual; it is plain that in any batc number that are of one kind or the other will be equal sum of those of the two kinds separately. Thus 36.4 i

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[1] It might be more accurate to speak of 'incompatible hypotheses with respect to any individual case', or 'mutually exclusive cla events'.

in 100 live to over sixty, 35.4 in 100 die before they are ten[1]; take a large number, say 10,000, then there will be about 3640 who live to over sixty, and about 3540 who do not reach ten; hence the total number who do not die within the assigned limits will be about 2820 altogether. Of course if these proportions were accurately assigned, the resultant sum would be equally accurate: but, as the reader knows, in Probability this proportion is merely the limit towards which the numbers tend in the long run, not the precise result assigned in any particular case. Hence we can only venture to say that this is the limit towards which we tend as the numbers become greater and greater.

This rule, in its general algebraic form, would be expressed in the language of Probability as follows:—If the chances of two exclusive or incompatible events be respectively $\frac{1}{m}$ and $\frac{1}{n}$ the chance of one or other of them happening will be $\frac{1}{m} + \frac{1}{n}$ or $\frac{m+n}{mn}$. Similarly if there were more than two events of the kind in question. On the principles adopted in this work, the rule, when thus algebraically expressed, means precisely the same thing as when it is expressed in the statistical form. It was shown at the conclusion of the last chapter that to say, for example, that the chance of a given event happening in a certain way is $\frac{1}{6}$, is only another way of saying that in the long run it does tend to happen in that way once in six times.

It is plain that a sort of corollary to this rule might be obtained, in precisely the same way, by subtraction instea addition. Stated generally it would be as follows:—If chance of one or other of two incompatible events be $\frac{1}{m}$ the chance of one alone be $\frac{1}{n}$, the chance of the remai one will be $\frac{1}{m} - \frac{1}{n}$ or $\frac{n-m}{nm}$.

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[1] The examples, of this kind, referring to human mortality are taken from the Carlisle tables. These differ considerably, as is well known, from other tables, but we have the high authority of De Morgan for regarding them as the best representative of the average mortality of the English middle classes at the present day.

For example, if the chance of any one dying in a ye $\frac{1}{10}$, and his chance of dying of some particular disease is $\frac{}{1}$ his chance of dying of any other disease is $\frac{9}{100}$.

The reader will remark here that there are two appare different modes of stating this rule, according as we sp of 'one or other of two or more events happening,' or of ' same event happening in one or other of two or more w But no confusion need arise on this ground; either wa speaking is legitimate, the difference being merely ver and depending (as was shown in the first chapter, § 8) u whether the distinctions between the 'ways' are or are too deep and numerous to entitle the event to be con tionally regarded as the same.

We may also here point out the justification for the c mon doctrine that certainty is represented by unity, jus any given degree of probability is represented by its ap priate fraction. If the statement that an event happens in m times, is equivalently expressed by saying that its ch is $\frac{1}{m}$, it follows that to say that it happens m times i times, or every time without exception, is equivalen saying that its chance is $\frac{m}{m}$ or 1. Now an event that hap every time is of course one of whose occurrence we rtain; hence the fraction which represents the 'chance' of event which is certain becomes unity.

It will be equally obvious that given that the chance that n event will happen is $\frac{1}{m}$, the chance that it will not happen $1 - \frac{1}{m}$ or $\frac{m-1}{m}$.

§ 3. (2) We can also make inferences by multiplication division. Suppose that the two events, instead of being compatible, are connected together in the sense that one contingent upon the occurrence of the other. Let us be ld that a given proportion of the members of the series ssess a certain property, and a given proportion again of ese possess another property, then the proportion of the ole which possess both properties will be found by multiying together the two fractions which represent the above o proportions. Of the inhabitants of London, twenty-five a thousand, say, will die in the course of the year; we ppose it to be known also that one death in five is due to ver; we should then infer that one in 200 of the inhabitants ll die of fever in the course of the year. It would of course equally simple, by division, to make a sort of converse ference. Given the total mortality per cent. of the populaon from fever, and the proportion of fever cases to the gregate of other cases of mortality, we might have inferred, dividing one fraction by the other, what was the total ortality per cent. from all causes.

The rule as given above is variously expressed in the nguage of Probability. Perhaps the simplest and best atement is that it gives us the rule of dependent events. hat is; if the chance of one event is $\frac{1}{m}$, and the chance that it happens another will also happen is $\frac{1}{n}$, then the chance of the latter is $\frac{1}{mn}$. In this case it is assumed that the lat is so entirely dependent upon the former that though it d not always happen with it, it certainly will not happen wi out it; the necessity of this assumption however may obviated by saying that what we are speaking of in latter case is the *joint* event, viz. both together if they simultaneous events, or the latter *in consequence of* former, if they are successive.

§ 4. The above inferences are necessary, in the sens which arithmetical inferences are necessary, and they do demand for their establishment any arbitrary hypoth We assume in them no more than is warranted, and in necessitated by the data actually given to us, and make inferences from these data by the help of arithmetic. In simple examples given above nothing is required bey arithmetic in its most familiar form, but it need hardly added that in practice examples may often present the selves which will require much profounder methods th these. It may task all the resources of that higher and m abstract arithmetic known as algebra to extract a soluti But as the necessity of appeal to such methods as these d not touch the principles of this part of the subject we ne not enter upon them here.

§ 5. The formula next to be discussed stands upo somewhat different footing from the above in respect of cogency and freedom from appeal to experience, or to hy thesis. In the two former instances we considered cases which the data were supposed to be given under the conditi that the properties which distinguished the different kind events whose frequency was discussed, were respecti known to be disconnected and known to be connected. us now suppose that no such conditions are given to e man in ten, say, has black hair, and one in twelve hort-sighted; what conclusions could we then draw as the chance of any given man having one only of these attributes, or neither, or both? It is clearly possible t the properties in question might be inconsistent with another, so as never to be found combined in the same son; or all the short-sighted might have black hair; or properties might be allotted[1] in almost any other propor- n whatever. If we are perfectly ignorant upon these nts, it would seem that no inferences whatever could be wn about the required chances.

Inferences however *are* drawn, and practically, in most es, quite justly drawn. An escape from the apparent eterminateness of the problem, as above described, is nd by assuming that, not merely will one-tenth of the ole number of men have black hair (for this was given as e of the data), but also that one-tenth alike of those who and who are not short-sighted have black hair. Let us ke a batch of 1200, as a sample of the whole. Now, from e data which were originally given to us, it will easily be n that in every such batch there will be on the average 0 who have black hair, and therefore 1080 who have not. d here in strict right we ought to stop, at least until we ve appealed again to experience; but we do not stop here. om data which we assume, we go on to infer that of the 0, 10 (i.e. one-twelfth of 120) will be short-sighted, and 0 (the remainder) will not. Similarly we infer that of the

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[1] I say, *almost* any proportion, be- se, as may easily be seen, arith- ic imposes certain restrictions n the assumptions that can be le. We could not, for instance, pose that all the black-haired men are short-sighted, for in any given batch of men the former are more numerous. But the range of these restrictions is limited, and their existence is not of importance in the above discussion.

1080, 90 are short-sighted, and 990 are not. On the whole, then, the 1200 are thus divided:—black-haired short-sighted, 10; short-sighted without black hair, 90; black-haired men who are not short-sighted, 110; men who are neither short-sighted nor have black hair, 990.

This rule, expressed in its most general form, in the language of Probability, would be as follows:—If the chances of a thing being p and q are respectively $\frac{1}{m}$ and $\frac{1}{n}$, then the chance of its being both p and q is $\frac{1}{mn}$, p and not q is $\frac{n-1}{mn}$, q and not p is $\frac{m-1}{mn}$, not p and not q is $\frac{(m-1)(n-1)}{mn}$, where p and q are independent. The sum of these chances is obviously unity; as it ought to be, since one or other the four alternatives must necessarily exist.

§ 6. I have purposely emphasized the distinction between the inference in this case, and that in the two preceding, to an extent which to many readers may seem unwarranted. But it appears to me that where a science makes use, as Probability does, of two such very distinct sources conviction as the necessary rules of arithmetic and the merely more or less cogent ones of Induction, it is hardly possible to lay too much stress upon the distinction. Fe will be prepared to deny that very arbitrary assumptio have been made by many writers on the subject, and no will deny that in the case of what are called 'inverse prob bilities' assumptions are sometimes made which are at lea decidedly open to question. The best course therefore is t make a pause and stringent enquiry at the point at which th possibility of such error and doubtfulness first exhibits itself. These remarks apply to some of the best writers on the subject; in the case of inferior writers, or those who appeal t obability without having properly mastered its principles, may go further. It would really not be asserting too uch to say that they seem to think themselves justified in suming that where we know nothing about the distribution the properties alluded to we must assume them to be dis- ibuted as above described, and therefore apportion our lief in the same ratio. This is called 'assuming the events be independent,' the supposition being made that the rule ill certainly follow from this independence, and that we ve a right, if we know nothing to the contrary, to assume at the events are independent.

The validity of this last claim has already been discussed the first chapter; it is only another of the attempts to nstruct *à priori* the series which experience will present to , and one for which no such strong defence can be made as r the equality of heads and tails in the throws of a penny. ut the meaning to be assigned to the 'independence' of the ents in question demands a moment's consideration.

The circumstances of the problem are these. There are o different qualities, by the presence and absence respec- vely of each of which, amongst the individuals of a series, o distinct pairs of classes of these individuals are pro- uced. For the establishment of the rule under discussion was found that one supposition was both necessary and fficient, namely, that the division into classes caused by ch of the above distinctions should subdivide each of the asses created by the other distinction in the same ratio which it subdivides the whole. If the independence be anted and so defined as to mean this, the rule of course ill stand, but, without especial attention being drawn to e point, it does not seem that the word would naturally so understood.

§ 7. The above, then, being the fundamental rules of inference in probability, the question at once arises, What is their relation to the great body of formulæ which are made use of in treatises upon the science, and in practical applications of it? The reply would be that these formulæ, in so far as they properly belong to the science, are nothing else in reality than applications of the above fundamental rules. Such applications may assume any degree of complexity, but owing to the difficulty of particular examples, in the form in which they actually present themselves, recourse must sometimes be made to the profoundest theorems of mathematics. Still we ought not to regard these theorems as being anything else than convenient and necessary abbreviations of arithmetical processes, which in practice have become too cumbersome to be otherwise performed.

This explanation will account for some of the rules as they are ordinarily given, but by no means for all of them. It will account for those which are demonstrable by the certain laws of arithmetic, but not for those which in reality rest only upon inductive generalizations. And it can hardly be doubted that many rules of the latter description have become associated with those of the former, so that in popular estimation they have been blended into one system, of which all the separate rules are supposed to possess a similar origin and equal certainty. Hints have already been frequently given of this tendency, but the subject is one of such extreme importance that a separate chapter (that on Induction) must be devoted to its consideration.

§ 8. In establishing the validity of the above rules, we have taken as the basis of our investigations, in accordance with the general scheme of this work, the statistical frequency of the events referred to; but it was also shown that each formula, when established, might with equal propriety be expressed in the more familiar form of a fraction representing

e 'chance' of the occurrence of the particular event. The
estion may therefore now be raised, Can those writers who
described in the last chapter) take as the primary subject
the science not the degree of statistical frequency, but the
ntity of belief, with equal consistency make this the basis
their rules, and so also regard the fraction expressive of
chance as a merely synonymous expression? De Morgan
intains that whereas in ordinary logic we suppose the
mises to be absolutely true, the province of Probability is
study 'the effect which partial belief of the premises pro-
ces with respect to the conclusion.' It would appear
refore as if in strictness we ought on this view to be able
determine this consequent diminution at first hand, from
rospection of the mind, that is of the conceptions and
iefs which it entertains; instead of making any recourse
statistics to tell us how much we ought to believe the
clusion.

Any readers who have concurred with me in the general
ults of the last chapter, will naturally agree in the conclu-
n that nothing deserving the name of logical science can
extracted from any results of appeal to our consciousness
to the quantity of belief we entertain of this or that pro-
ition. Suppose, for example, that one person in 100 dies
the sea passage out to India, and that one in 9 dies dur-
a 5 years residence there. It would commonly be said
t the chance that any one, who is now going out, has of
ing to start homewards 5 years hence, is $\frac{88}{100}$; for his chance
getting there is $\frac{99}{100}$; and of his surviving, if he gets
re, $\frac{8}{9}$; hence the result or dependent event is got by
ltiplying these fractions together, which gives $\frac{88}{100}$. Here
real basis of the reasoning is statistical, and the processes
results are merely translated afterwards into fractions.
t can we say the same when we look at the belief side of

the question? I quite admit the psychological fact th
have degrees of belief, more or less corresponding t
frequency of the events to which they refer. In the abo
ample, for instance, we should undoubtedly admit on en
that our belief in the man's return was affected by ea
the risks in question, so that we had less expectation
than if he were subject to either risk separately; that i
should in some way compound the risks. But what I c
recognise is that we should be able to perform the p
with any approach to accuracy without appeal to the s
tics, or that, even supposing we could do so, we s
have any guarantee of the correctness of the result
out similar appeal. It appears to me in fact that but
meaning, and certainly no security, can be attained
regarding the process of inference. The probabilitie
pressed as degrees of belief, just as those which are exp
as fractions, must, when we are put upon our justific
first be translated into their corresponding facts of stati
frequency of occurrence of the events, and then th
ferences must be drawn and justified there. This pa
the operation, as we have already shown, is mostly ca
on by the ordinary rules of arithmetic. When we
obtained our conclusion we may, if we please, transla
back again into the subjective form, just as we can a
for convenience into the fractional, but I do not see ho
process of inference can be conceived as taking place in
form, and still less how any proof of it can thus be given
therefore the process of inference be so expressed it mu
regarded as a symbolical process, symbolical of such a
ference about things as has been described above, a
therefore seems to me more advisable to state and exp
it in this latter form.

On Inverse Probability and the Rules required for it.

§ 9. It has been already stated that the only funda-
ntal rules of inference in Probability are the two described
§§ 2, 3, but there are of course abundance of derivative
es, the nature and use of which are best obtained from the
dy of any manual upon the subject. One class of these
ivative rules, however, is sufficiently distinct in respect of
questions to which it may give rise, to deserve special
mination. It involves the distinction commonly recog-
ed as that between Direct and Inverse Probability. It is
s introduced by De Morgan :—

"In the preceding chapter we have calculated the chances
n event, knowing the circumstances under which it is to
pen or fail. We are now to place ourselves in an inverted
ition: we know the event, and ask what is the probability
ich results from the event in favour of any set of circum-
nces under which the same might have happened[1]." The
tinction might therefore be summarily described as that
tween finding an effect when we are given the causes, and
ding a cause when we are given effects.

On the principles of the science involved in the definition
ich was discussed and adopted in the earlier chapters of
work, the reader will easily infer that no such distinction
this can be regarded as fundamental. One common feature
traced in all the objects which were to be referred to
obability, and from this feature the possible rules of

Essay on Probabilities, p. 53. I
been reminded that in his arti-
on Probability in the *Encyclopædia Metropolitana* he has stated that such rules involve no new principle.

inference can be immediately derived. All other distinctions are merely those of arrangement or management.

But although the distinction is not by any means fundamental, it is nevertheless true that the practical treatment of such problems as those principally occurring in Inverse Probability, does correspond to a very serious source o ambiguity and perplexity. The arbitrary assumptions whic appear in Direct Probability are not by any means serious; but those which invade us in a large proportion of the problems offered by Inverse Probability are both serious an inevitable.

§ 10. This will be best seen by the examination o special examples; as any, however simple, will serve ou purpose, let us take the two following:—

(1) A ball is drawn from a bag containing nine blac balls and one white: what is the chance of its being th white ball?

(2) A ball is drawn from a bag containing ten balls, an is found to be white; what is the chance of there havin been but that one white ball in the bag?

The class of which the first example is a simple instanc has been already abundantly discussed. The interpretati of it is as follows: If balls be continually drawn and re placed, the proportion of white ones to the whole numb drawn will tend towards the fraction $\frac{1}{10}$. The contempla action is a single one, but we view it as one of the abo series; at least our opinion is formed upon that assumpti We conclude that we are going to take one of a series o events which may appear individually fortuitous, but i which, in the long run, those of a given kind are one-tenth o the whole; this kind (white) is then singled out by antici tion. By stating that its chance is $\frac{1}{10}$, we merely mean t assert this physical fact, together with such other ment

cts, emotions, inferences, &c., as may be properly associ-
ted with it.

§ 11. Have we to interpret the second example in a
fferent way? Here also we have a single instance, but the
ture of the question would seem to decide that the only
ies to which it can properly be referred is the following:—
lls are continually drawn from *different* bags each contain-
g ten, and are always found to be white; what is ultimately
e proportion of cases in which they will be found to have
en taken from bags with only one white ball in them?
ow it may be readily shown[1] that time has nothing to
with the question; omitting therefore the consideration
this element, we have for the two series from which our
inions in these two examples respectively are to be
med:—(1) balls of different colours presented to us in a
ven ultimate ratio; (2) bags with different contents simi-
ly presented. From these data respectively we have to
ign their due weight to our anticipations of (1) a white
ll; (2) a bag containing but one white ball. So stated the
oblems would appear to be formally identical.

When, however, we begin the practical work of solving
em we perceive a most important distinction. In the first
mple there is not much that is arbitrary; balls would
der such circumstance really come out more or less accu-
ely in the proportion expected. Moreover, in case it
uld be objected that it is difficult to prove that they will
so, it does not seem an unfair demand to say that the balls
to be 'well-mixed' or 'fairly distributed,' or to introduce
of the other conditions by which, under the semblance of
ing *à priori*, we take care to secure our prospect of a

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[1] his point will be fully discussed
uture chapter, after the general
l-point of an objective system of logic has been explained and illustrated.

series of the desired kind. But we cannot say the sam the case of the second example.

§ 12. The line of proof by which it is generally tempted to solve the second example is of this kind;— shown that there being one white ball for certain in the the only possible antecedents are of ten kinds, viz. b each of which contains ten balls, but in which the w balls range respectively from one to ten in number. Th course imposes limits upon the kind of terms to be fo in our series. But we want more than such limitations must know the proportions in which these terms are mately found to arrange themselves in the series. Now requires an experience about bags which may not, and deed in a large proportion of similar cases, cannot, be g to us. If therefore we are to solve the question at all must make an assumption; let us make the following;— *each of the bags described above occurs equally often,*—and what follows. The bags being drawn from equally ofte does not follow that they will each yield equal numbe white balls. On the contrary they will, as in the example, yield them in direct proportion to the numbe such balls which they contain. The bag with one w and nine black will yield a white ball once in ten times; with two white, twice; and so on. The result of this, it be easily seen, is that in 100 drawings there will be obtai on the average 55 white balls and 45 black. Now those drawings that do not yield white balls we have, by question, nothing to do, for that question postulated drawing of a white ball as an accomplished fact. The s we want is therefore composed of those which do yield wh Now what is the additional attribute which is found in s members, and in some members only, of this series, which we mentally anticipate? Clearly it is the attribut ving been drawn from a bag which only contained one of ese white balls. Of these there is, out of the 55 drawings, t one. Accordingly the required chance is $\frac{1}{55}$. That is to y, the white ball will have been drawn from the bag con- ining only that one white, once in 55 times.

§ 13. Now, with the exception of the passage in italics, e process here is precisely the same as in the other exam- e; it is somewhat longer only because we are not able to peal immediately to experience, but are forced to try to duce what the result will be, though the validity of this duction itself rests, of course, ultimately upon experience. t the above passage is a very important one. It is scarcely cessary to point out how arbitrary it is.

For *is* the supposition, that the different specified kinds bags are equally likely, the most reasonable supposition der the circumstances in question? One man may think is, another may take a contrary view. In fact in an excel- nt manual[1] upon the subject a totally different supposition made, at any rate in one example; it is taken for granted that instance, not that every possible number of black and hite balls respectively is equally likely, but that every sible way of getting each number is equally likely, whence follows that bags with an intermediate number of black d white balls are far more likely than those with an ex- eme number of either. On this supposition five black d five white being obtainable in 252 ways against the n ways of obtaining one white and nine black, it fol- ws that the chance that we have drawn from a bag of e latter description is much less than on the hypothesis st made. The chance, in fact, becomes now $\frac{1}{512}$ instead $\frac{1}{55}$. In the one case each distinct result is considered equally likely, in the other every distinct way of g each result.

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[1] Whitworth's *Choice and Chance*, Ed. II., p. 123. See also Boole's *ws of Thought*, p. 370.

§ 14. Uncertainties of this kind are peculiarly lil arise in these inverse probabilities, because when merely given an effect and told to look out for the ch some assigned cause, we are often given no clue as to th tive prevalence of these causes, but are left to determin on general principles. Give us either their actual prev in statistics, or the conditions by which such prevale brought about, and we know what to do; but witho help of such data we are reduced to guessing. In the example, if we had been told how the bag had been ori filled, that is by what process, or under what circums we should have known what to do. If it had been fi random from a box containing equal numbers of bla white balls, the supposition in Mr Whitworth's exa the most reasonable; but in the absence of any such mation as this we are entirely in the dark, and the su tion made in § 12 is neither more nor less trustwort reasonable than many others, though it doubtless po the merit of superior simplicity[1]. If the reader will r Ch. v. §§ 4, 5, he will find this particular difficult explained. Everybody practically admits that a characteristic arrangement or distribution has to be duced at some prior stage; and that, as soon as thi has been selected, there are no further theoretic diff to be encountered. But when we come to decide, in ex of the class in question, at what stage it is most rea make our postulate, we are often left without any very efinite or rational guidance.

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[1] Opinions differ about the defence of such suppositions, as they do about the nature of them. Some writers, admitting the above assumption to be doubtful, call it the most i hypothesis. Others regard sort of mean hypothesis.

§ 15. When, however, we take what may be called, by mparison with the above purely artificial examples, instances esented by nature, much of this uncertainty will disappear, d then all real distinction between direct and inverse obability will often vanish. In such cases the causes are ostly determined by tolerably definite rules, instead of eing a mere cloud-land of capricious guesses. We may ther find their relative frequency of occurrence by referce to tables, or may be able to infer it by examination of e circumstances under which they are brought about. lmost any simple example would then serve to illustrate e fact that under such circumstances the distinction etween direct and inverse probability disappears altogether, r merely resolves itself into one of *time*, which, as will be ore fully shown in a future chapter, is entirely foreign to ur subject.

It is not of course intended to imply that difficulties imilar to those mentioned above do not occasionally invade s here also. As already mentioned, they are, if not inherent n the subject, at any rate almost unavoidable in comparison ith the simpler and more direct procedure of determining hat is likely to follow from assigned conditions. What is eant is that so long as we confine ourselves within the omparatively regular and uniform field of natural sequences nd coexistences, statistics of causes may be just as readily vailable as those of effects. There will not be much more hat is arbitrary in the one than in the other. But of course his security is lost when, as will be almost immediately oticed, what may be called metaphysical rather than na-ural causes are introduced into the enquiry.

For instance, it is known that in London about 20 people

die per thousand each year. Suppose it also known th
every 100 deaths there are about 4 attributable to bronc
The odds therefore against any unknown person dyin
bronchitis in a given year are 1249 to 1. Exactly the s
statistics are available to solve the inverse problem:—A
is dead, what is the chance that he died of bronchitis? H
since the man's death is taken for granted, we do not req
to know the general average mortality. All that we wa
the proportional mortality from the disease in questio
given above. If Probability dealt only with infere
founded in this way upon actual statistics, and these t
ably extensive, it is scarcely likely that any distinction s
as this between direct and inverse problems would ever h
been drawn.

§ 16. Considered therefore as a contribution to the th
of the subject, the distinction between Direct and Inverse
bability must be abandoned. When the appropriate st
tics are at hand the two classes of problems become ident
in method of treatment, and when they are not we have
more right to extract a solution in one case than in the ot
The discussion however may serve to direct renewed at
tion to another and far more important distinction. It
remind us that there is one class of examples to which
calculus of Probability is rightfully applied, because statist
data are all we have to judge by; whereas there are o
examples in regard to which, if we will insist upon mak
use of these rules, we may either be deliberately abando
the opportunity of getting far more trustworthy informa
by other means, or we may be obtaining solutions a
matters on which the human intellect has no right to
definite quantitative opinion.

§ 17. The nearest approach to any practical justifica
of such judgments that I remember to have seen is affo

cases of which the following example is a specimen:—Of 10 cases treated by Lister's method, 7 did well and 3 ffered from blood-poisoning: of 14 treated with ordinary essings, 9 did well and 5 had blood-poisoning; what are e odds that the success of Lister's method was due to nce?[1]". Or, to put it into other words, a short experience s shown an actual superiority in one method over the her: what are the chances that an indefinitely long expe-ence, under similar conditions, will confirm this superiority? The proposer treated this as a 'bag and balls' problem, alogous to the following: 10 balls from one bag gave white and 3 black, 14 from another bag gave 9 white and black: what is the chance that the actual ratio of white to ack balls was greater in the former than in the latter?—is actual ratio being of course considered a true indication what would be the ultimate proportions of white and black awings. This seems to me to be the only reasonable way treating the problem, if it is to be considered capable of umerical solution at all.

Of course the inevitable assumption has to be made here bout the equal prevalence of the different possible kinds of g,—or, as the supporters of the justice of the calculation ould put it, of the obligation to assume the equal *à priori* kelihood of each kind,—but I think that in this particular ample the arbitrariness of the assumption is less than ual. This is because the problem discusses simply a lance between two extremely similar cases, and there is a rtain set-off against each other of the objectionable assumptions on each side. Had *one* set of experiments only b proposed, and had we been asked to evaluate the probabil of continued repetition of them confirming their verdic should have felt all the scruples I have already mentio But here we have got two sets of experiments carried under almost exactly similar circumstances, and there therefore less arbitrariness in assuming that their unkno conditions are tolerably equally prevalent.

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[1] *Educational Times;* Reprint, Vol. xvii. p. 40. The question was oposed by Dr Macalister and gave e to considerable controversy. As ual with problems of this inverse kind hardly any two of the writers were in agreement as to the assumptions to be made, or therefore as to the numerical estimate of the odds.

§ 18. Examples of the description commonly introdu seem objectionable enough, but if we wish to realize th full extent the vagueness of some of the problems subm ted to this Inverse Probability, we have not far to seek. natural as in artificial examples, where statistics are unatta able the enquiry becomes utterly hopeless, and all attem at laying down rules for calculation must be abando Take, for instance, the question which has given rise to s discussion[1], whether such and such groups of stars are or not to be regarded as the results of an accidental distri tion; or the still wider and vaguer question, whether such such things, or say the world itself, have been produced chance?

In cases of this kind the insuperable difficulty is in de mining what sense exactly is to be attached to the w 'accidental' and 'random' which enter into the discuss Some account was given, in the fourth chapter, of t scientific and conventional meaning in Probability. T seem to be the same objections to generalizing them ou such relation, as there is in metaphysics to talking of Infinite or the Absolute. Infinite magnitude, or infi wer, one can to some extent comprehend, or at least one y understand what is being talked about, but '*the* infi- e' seems to me a term devoid of meaning. So of anything posed to have been produced at random: tell us the ure of the agency, the limits of its randomness and so on, l we can venture upon the problem, but without such data know not what to do. The further consideration of such roblem might, I think, without arrogance be relegated to Chapter on Fallacies. Accordingly any further remarks ich I have to make upon the subject will be found there, l at the conclusion of the chapter on Causation and sign.

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[1] See Todhunter's *History*, pp. 333, 4.

There is an interesting discussion upon this question by the late J. D. Forbes in a paper in the *Philo cal Magazine* for Dec. 1850. It replied to in a subsequent numb Prof. Donkin.

CHAPTER VIII.

THE RULE OF SUCCESSION[1].

§ 1. In the last chapter we discussed at some lengt nature of the kinds of inference in Probability which spond to those termed, in Logic, immediate and mediate ences. We ascertained what was the meaning of sayin example, that the chance of any given man A. B. in a year is $\frac{1}{3}$, when concluded from the general propo that one man out of three in his circumstances dies. also discussed the nature and evidence of rules of a completely inferential character. But to stop at this would be to take a very imperfect view of the subjec Probability is a science of real inference about thin must surely lead up to something more than such m formal conclusions; we must be able, if not by means of any rate by some means, to step beyond the limits of has been actually observed, and to draw conclusions what is as yet unobserved. This leads at once to the tion, What is the connection of Probability with Induc This is a question into which it will be necessary to now with some minuteness.

That there is a close connection between Probabilit Induction, must have been observed by almost every ho has treated of either subject; I have not however seen y account of this connection that seemed to me to be tisfactory. An explicit description of it should rather be ught in treatises upon the narrower subject, Probability; t it is precisely here that the most confusion is to be und. The province of Probability being somewhat narrow, cursions have been constantly made from it into the ad- cent territory of Induction. In this way, amongst the ithmetical rules discussed in the last chapter, others have en frequently introduced which ought not in strictness to classed with them, as they rest on an entirely different is.

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[1] A word of apology may be offered here for the introduction of a new name. The only other alternative would have been to entitle the rule one of *Induction*. But such a cannot admit, for reasons whi be almost immediately explain

§ 2. The origin of such confusion is easy of explana- n; it arises, doubtless, from the habit of laying undue ss upon the *subjective* side of Probability, upon that ich treats of the quantity of our belief upon different bjects and the variations of which that quantity is sus- ptible. It has been already urged that this variation of lief is at most but a constant accompaniment of what is lly essential to Probability, and is moreover common to her subjects as well. By defining the science therefore om this side these other subjects would claim admittance to it; some of these, as Induction, have been accepted, but hers have been somewhat arbitrarily rejected. Our belief a wider proposition gained by Induction is, prior to verifi- tion, not so strong as that of the narrower generalization om which it is inferred. This being observed, a so-called le of probability has been given by which it is supposed at this diminution of assent could in many instances be lculated.

But *time* also works changes in our conviction; our belief the happening of almost every event, if we recur to it long terwards, when the evidence has faded from the mind, is less strong than it was at the time. Why are not ru oblivion inserted in treatises upon Probability? If a n told how firmly he ought to expect the tide to rise a because it has already risen ten times, might he not als for a rule which should tell him how firm should be his of an event which rests upon a ten years' recollection?[1] infractions of a rule of this latter kind could scarcely be numerous and extensive, as we shall see presently, than of the former confessedly are. The fact is that the age by which the strength of our conviction is modified, a indefinitely numerous that they cannot all be assembled one science; for purposes of definition therefore the qua of belief had better be omitted from consideration, or a rate regarded as a mere appendage, and the science, de from the other or statistical side of the subject, in w as has been shown, a tolerably clear boundary-line ca traced.

§ 3. Induction, however, from its importance does r a separate discussion; a single example will show its be upon this part of our subject. We are considering the pect of a given man, A.B. living another year, and we that nine out of ten men of his age do survive. In forı an opinion about his surviving, however, we shall find there are in reality two very distinct causes which a determining the strength of our conviction; distinct, bu practice so intimately connected that we are very ap overlook one, and attribute the effect entirely to the oth

(I) There is that which strictly belongs to Probabi t which (as was explained in Chap. VI.) measures our lief of the individual case as deduced from the general position. Granted that nine men out of ten of the kind which A. B. belongs do live another year, it obviously s not follow at all necessarily that *he* will. We describe state of things by saying, that our belief of his surviving diminished from certainty in the ratio of 10 to 9, or, in er words, is measured by the fraction $\frac{9}{10}$.

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[1] John Craig, in his often named work, *Theologiæ Christianæ Principia Mathematica* (Lond. 1699) attempted something in this direction when he proposed to solve such problems as:—Quando evanesc babilitas cujusvis Historiæ, cuju jectum est transiens, vivâ ta voce transmissæ, determinare.

(II) But are we certain that nine men out of ten like n *will* live another year? we know that they have so sured in time past, but will they continue to do so? Since B. is still alive it is plain that this proposition is to a tain extent assumed, or rather obtained by Induction. e cannot however be as certain of the inductive inference we are of the data from which it was inferred. Here, refore, is a second cause which tends to diminish our lief; in practice these two causes always accompany each her, but in thought they can be separated.

The two distinct causes described above are very liable be confused together, and the class of cases from which mples are necessarily for the most part drawn increases s liability. The step from the statement 'all men have d in a certain proportion' to the inference 'they will conue to die in that proportion' is so slight a step that it is noticed, and the diminution of conviction that should company it is unsuspected. In what are called *à priori* amples the step is still slighter. We feel so certain about e permanence of the laws of mechanics, that few people uld think of regarding it as an inference when they lieve that a die will in the long run turn up all its faces ually often, because other dice have done so in time st.

§ 4. It has been already pointed out (in Chapter VI.)

that, so far as concerns that definition of Probability wh regards it as the science which discusses the degree modifications of our belief, the question at issue seems t simply this:—Are the causes alluded to above in (II) cap of being reduced to one simple coherent scheme, so that universal rules for the modification of assent can be obtai from them? If they are, strong grounds will have b shown for classing them with (I), in other words, for c sidering them as rules of probability. Even then t would be rules practically of a very different kind, con gent instead of necessary (if one may use these terms w out committing oneself to any philosophical system), but objection might perhaps be overruled by the greater sim city secured by classing them together. This view is, w various modifications, generally adopted by writers on P bability, or at least, as I understand the matter, implied their methods of definition and treatment. Or, on the o hand, must these causes be regarded as a vast system, might almost say a chaos, of perfectly distinct agenc which may indeed be classified and arranged to some ext but from which we can never hope to obtain any rule perfect generality which shall not be subject to const exception? If so, but one course is left; to exclude t all alike from Probability. In other words, we must assu the general proposition, viz. that which has been descri throughout as our starting-point, to be given to us; it be obtained by any of the numerous rules furnished Induction, or it may be inferred deductively, or given by own observation; its value may be diminished by its dep ing upon the testimony of witnesses, or its being recalled our own memory. Its real value may be influenced these causes or any combinations of them; but all these preliminary questions with which we have nothing dir

do. We assume our statistical proposition to be true,
glecting the diminution of its value by the process of
ainment; we take it up first at this point and then apply
r rules to it. We receive it in fact, if one may use the
pression, *ready-made*, and ask no questions about the pro-
s or completeness of its manufacture.

§ 5. It is not to be supposed, of course, that any writers
ve seriously attempted to reduce to one system of calcula-
n all the causes mentioned above, and to embrace in one
mula the diminution of certainty to which the inclusion of
em subjects us. But on the other hand, they have been
willing to restrain themselves from all appeal to them.
m an early period in the study of the science attempts
ve been made to proceed, by the Calculus of Probability,
m the observed cases to adjacent and similar cases. In
ctice, as has been already said, it is not possible to avoid
he extension of this kind. But it should be observed
t in these instances the divergence from the strict ground
experience is not in reality recognized, at least not as a
t of our logical procedure. We have, it is true, wandered
newhat beyond it, and so obtained a wider proposition
n our data strictly necessitated, and therefore one of less
tainty. Still we assume the conclusion given by induc-
n to be equally certain with the data, or rather omit all
ice of the divergence from consideration. It is assumed
t the unexamined instances will resemble the examined,
assumption for which abundant warrant may exist; the
ory of the calculation rests upon the supposition that
re will be no difference between them, and the practical
or is insignificant simply because this difference is small.

§ 6. But the rule we are now about to discuss, and
ich may be called the Rule of Succession, is of a very
erent kind. It not only recognizes the fact that we are

leaving the ground of past experience, but takes the c
quences of this divergence as the express subject of its
lation. It professes to give a general rule for the meas
expectation that we should have of the reappearance
phenomenon that has been already observed any num
times. This rule is generally stated somewhat as fol
"To find the chance of the recurrence of an event al
observed, divide the number of times the event has
observed, increased by one, by the same number incr
by two."

§ 7. It will be instructive to point out the orig
this rule; if only to remind the reader of the necessi
keeping mathematical formulæ to their proper pro
and to show what astonishing conclusions are a
be accepted on the supposed warrant of mathem
Revert then to the example of Inverse Probability
182. We saw that under certain assumptions, it
follow that when a single white ball had been
from a bag known to contain 10 balls which were
or black, the chance could be determined that ther
only one white ball in it. Having done this we r
calculate 'directly' the chance that this white ball
be drawn next time. Similarly we can reckon the ch
of there being two, three, &c. up to ten white balls
and determine on each of these suppositions the c
of a white ball being drawn next time. Adding
together we have the answer to the question:—a
ball has been drawn once from a bag known to co
ten balls, white or black; what is the chance of a s
time drawing a white ball?

So far only arithmetic is required. For the next st
need higher mathematics, and by its aid we solve
problem:—A white ball has been drawn m times fr

g which contains any number, we know not what, of ls each of which is white or black, find the chance of next drawing also yielding a white ball. The answer is

$$\frac{m+1}{m+2}.$$

Thus far mathematics. Then comes in the physical umption that the universe may be likened to such a bag the above, in the sense that the above rule may be plied to solve this question:—an event has been observed happen m times in a certain way, find the chance that ill happen in that way next time. Laplace, for instance, pointed out that at the date of the writing of his *Essai losophique*, the odds in favour of the sun's rising again the old assumption as to the age of the world) were 26,214 to 1. De Morgan says that a man who standing the bank of a river has seen ten ships pass by with flags uld judge it to be 11 to 1 that the next ship will also ry a flag.

§ 8. It is hard to take such a rule as this seriously, for re does not seem to be even that moderate confirmation it which we shall find to hold good in the case of the plication of abstract formulæ to the estimation of the dence of witnesses. If however its validity is to be dis-sed there appear to be two very distinct lines of enquiry ng which we may be led.

(1) In the first place we may take it for what it pro-ses to be, and for what it is commonly understood to viz. a rule which assigns the measure of expectation ought to entertain of the recurrence of the event under circumstances in question. Of course, on the view pted in this work, we insist on enquiring whether it is lly true that on the average events do thus repeat their rformance in accordance with this law. Thus tested, no

one surely would attempt to defend such a formu far from past occurrence being a ground for belief in recurrence, there are (as will be more fully point in the Chapter on Fallacies) plenty of cases in whi direct contrary holds good. Then again a rule of th is subject to the very serious perplexity to be ex in our next chapter, arising out of the necessary arbitr of such inverse reference. That is, when an eve happened but a few times, we have no certain guid when it has happened but once[1], we have no guide wh as to the class of cases to which it is to be referre the example above, about the flags, why did we stop s this notion simply, instead of specifying the size, sha of the flags?

De Morgan, it must be remembered, only accep rule in a qualified sense. He regards it as furnis *minimum* value for the amount of our expectatio terms it "the rule of probability of a *pure inductio* says of it, "The probabilities shown by the above are merely *minima* which may be augmented by sources of knowledge." That is, he recognizes only instances in which our belief in the Uniformity of and in the existence of special laws of causation co supplement that which arises from the mere frequency past occurrence. This however does not meet those cases which past occurrence is a positive ground of disbelief in ure recurrence.

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[1] When $m=1$ the fraction becomes $\frac{2}{3}$; *i.e.* the odds are 2 to 1 in favour of recurrence. And there are writers who accept this result. For instance, Jevons (*Principles of Science* p. 258) says "Thus on the first occasion on which a person sees a shark, and notices that it is accompanied by a little pilot fish, the odds are 2 to 1 that the next shark will be so accompanied." To say nothing of the fact that recognizing and naming the fish implies that they have often been seen before, how many of served characteristics of th 'event' are to be considered e Must the pilot precede; an same distance? Must we the latitude, the ocean, the the species of shark, as ma of repetition on the next o and so on. I cannot see Inductive problem can be e telligibly stated, for qua purposes, on the first occur any event.

§ 9. (2) There is however another and very different w which might be taken of such a rule. It is one, an ure recognition of which has very likely had much to with the acceptance which the rule has received.

What we might suppose ourselves to be thus expressing —not the measure of rational expectation which might be d by minds sufficiently advanced to be able to classify d to draw conscious inferences, but,—the law according to ich the primitive elements of belief were started and veloped. Of course such an interpretation as this would equivalent to quitting the province of Logic altogether d crossing over into that of Psychology; but it would be a fectly valid line of enquiry. We should be attempting thing more than a development of the researches of chner and his followers in psycho-physical measurement. ly then we ought, like them, not to start with any analogy a ballot box and its contents, but to base our enquiry careful determination of the actual mental phenomena perienced. We know how the law has been determined accordance with which the intensity of the feeling of ht varies with that of its objective source. We see how it possible to measure the growth of *memory* according to number of repetitions of a sentence or a succession mere syllables. In this latter case, for instance, we just experiments, and determine how much better a man can member any utterances after eight hearings than after ven[1].

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[1] See in *Mind* (x. 454) Mr Jacob's unt of the researches of Herr Ebbinghaus as described in his work *Ueber das Gedächtniss*.

Now this case furnishes a very close parallel to supposed attempt to measure the increase of intensit belief after repeated recurrence. That is, if it were pos to experiment in this order of mental phenomena, we o simply to repeat a phenomenon a certain number of ti and then ascertain by actual introspection or by some sim test, how fast the belief was increasing. Thus viewed problem seems to me a hopeless one. The difficulties serious enough, when we are trying to measure our sim sensations, of laying aside the effects of past training, an attempting, as it were, to leave the mind open and pas to mere reception of stimuli. But if we were to atte in this way to measure our belief these difficulties w become quite insuperable. We can no more divest selves of past training here than we can of intelligenc thought. I do not see how any one could possibly a classing the observed recurrences with others which he experienced, and of being thus guided by special anal and inductions instead of trusting solely to De Mor 'pure induction'. The same considerations tend to r another form of defence for the rule in question. urged, for instance, that we may at least resort to i those cases in which we are in entire ignorance as to number and nature of the antecedents. This is a positi which I can hardly conceive it possible that we should be reduced. However remote or exceptional may be phenomenon selected we may yet bring it into relation some accepted generalizations and thus draw our conclu from these rather than from purely *à priori* consi tions.

§ 10. Since then past acquisitions cannot be laid or allowed for, the only remaining resource would b experiment upon the infant mind. One would not

pronounce that any line of enquiry is impossible; but
difficulties would certainly be enormous. And interesting
the facts would be, supposing that we had succeeded
securing them, they would not be of the slightest im-
rtance in Logic. However the question were settled:—
ether, for instance, we proved that the sentiment or
otion of belief grew up slowly and gradually from a sort
zero point under the impress of repetition of experience;
whether we proved that a single occurrence produced
mplete belief in the repetition of the event, so that
perience gradually untaught us and weakened our con-
tions;—in no case would the mature mind gain any aid
to what it ought to believe.

I cannot but think that some such view as this must
casionally underlie the acceptance which this rule has re-
ived. For instance, Laplace, though unhesitatingly adopt-
g it as a real, that is, objective rule of inference, has gone
to so much physiological and psychological matter towards
e end of his discussion (*Essai philosophique*) as to suggest
at what he had in view was the natural history of belief
ther than its subsequent justification.

Again, the curious doctrine adopted by Jevons, that the
inciples of Induction rest entirely upon the theory of
obability,—a very different doctrine from that which is
nveyed by saying that all knowledge of facts is *probable*
ly, i.e. not necessary,—seems unintelligible except on some
ch interpretation. We shall have more to say on this
bject in our next chapter. It will be enough here to
nark that in our present reflective and rational stage
find that every inference in Probability involves some
peal to, or support from, Induction, but that it is im-
ssible to base either upon the other. However far back
try to push our way, and however disposed we might be

to account for our ultimate beliefs by Association, it s
to me that so long as we consider ourselves to be de
with rules of inference we must still distinguish bet
Induction and Probability.

CHAPTER IX.

INDUCTION AND ITS CONNECTION WITH PROBABILITY.

1. WE were occupied, during the last chapter, with the amination of a rule, the object of which was to enable to make inferences about instances as yet unexamined. was professedly, therefore, a rule of an inductive cha-cter. But, in the form in which it is commonly expressed, was found to fail utterly. It is reasonable therefore to quire at this point whether Probability is entirely a formal deductive science, or whether, on the other hand, we are ble, by means of it, to make valid inferences about instances yet unexamined. This question has been already in part swered by implication in the course of the last two chap-ers. It is proposed in the present chapter to devote a fuller vestigation to this subject, and to describe, as minutely as mits will allow, the nature of the connection between Probability and Induction. We shall find it advisable for clear-ess of conception to commence our enquiry at a somewhat rly stage. We will travel over the ground, however, as pidly as possible, until we approach the boundary of what an properly be termed Probability.

§ 2. Let us then conceive some one setting to work to investigate nature, under its broadest aspect, with the view of systematizing the facts of experience that are known, and thence (in case he should find that this is possible) discovering others which are at present unknown. He observe multitude of phenomena, physical and mental, contempo and successive. He enquires what connections are th between them? what rules can be found, so that som these things being observed I can infer others from th We suppose him, let it be observed, deliberately resolvin investigate the things themselves, and not to be tu aside by any prior enquiry as to there being laws u which the mind is compelled to judge of the things. may arise either from a disbelief in the existence of independent and necessary mental laws, and a conseq conviction that the mind is perfectly competent to obs and believe anything that experience offers, and sh believe nothing else, or simply from a preference for inv gations of the latter kind. In other words, we suppose to reject Formal Logic, and to apply himself to a stud objective existences.

It must not for a moment be supposed that we are doing more than conceiving a fictitious case for the pur of more vividly setting before the reader the nature of inductive process, the assumptions it has to make, and character of the materials to which it is applied. It is psychologically possible that any one should come to the st of nature with all his mental faculties in full perfection, void of all materials of knowledge, and free from any bi to the uniformities which might be found to prevail ar him. In practice, of course, the form and the matter—laws of belief or association, and the objects to which are applied—act and react upon one another, and ne can exist in any but a low degree without presupposing existence of the other. But the supposition is perfectly timate for the purpose of calling attention to the req ments of such a system of Logic, and is indeed nothing an what has to be done at almost every step in psycho-gical enquiry[1].

§ 3. His task at first might be conceived to be a slow d tedious one. It would consist of a gradual accumula-n of individual instances, as marked out from one another various points of distinction, and connected with one other by points of resemblance. These would have to be pectively distinguished and associated in the mind, and e consequent results would then be summed up in general opositions, from which inferences could afterwards be awn. These inferences could, of course, contain no new ts, they would only be repetitions of what he or others d previously observed. All that we should have so far ne would have been to make our classifications of things d then to appeal to them again. We should therefore be eping well within the province of ordinary logic, the pro-sses of which (whatever their ultimate explanation) may course always be expressed, in accordance with Aristotle's ictum, as ways of determining whether or not we can show at one given class is included wholly or partly within other, or excluded from it, as the case may be.

§ 4. But a very short course of observation would sug-st the possibility of a wide extension of his information. xperience itself would soon detect that events were con-cted together in a regular way; he would ascertain that ere are 'laws of nature.' Coming with no *à priori* neces-ity of believing in them, he would soon find that as a matter

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[1] Some of my readers may be fa-iliar with a very striking digression Buffon's Natural History (*Natural ist. of Man*, § VIII.), in which he pposes the first man in full pos-sion of his faculties, but with all his experience to gain, and speculates on the gradual acquisition of his knowledge. Whatever may be thought of his particular conclusions the passage is very interesting and suggestive to any student of Psychology.

of fact they do exist, though he could not feel any certai
as to the extent of their prevalence. The discovery of t
arrangement in nature would at once alter the plan of his p
ceedings, and set the tone to the whole range of his meth
of investigation. His main work now would be to find o
by what means he could best discover these laws of nature

An illustration may assist. Suppose I were engaged
breaking up a vast piece of rock, say slate, into small pie
I should begin by wearily working through it inch by in
But I should soon find the process completely changed ow
to the existence of *cleavage.* By this arrangement of thi
a very few blows would do the work—not, as I might p
sibly have at first supposed, to the extent of a few inche
but right through the whole mass. In other words, by t
process itself of cutting, as shown in experience, and
nothing else, a constitution would be detected in the thi
that would make that process vastly more easy and ext
sive. Such a discovery would of course change our tacti
Our principal object would thenceforth be to ascertain t
extent and direction of this cleavage.

Something resembling this is found in Induction. Th
discovery of laws of nature enables the mind to dart with i
inferences from a few facts completely through a whole cl
of objects, and thus to acquire results the successive indiv
dual attainment of which would have involved long an
wearisome investigation, and would indeed in multitud
of instances have been out of the question. We have n
demonstrative proof that this state of things is universa
but having found it prevail extensively, we go on with th
resolution at least to try for it everywhere else, and we a
not disappointed. From propositions obtained in this wa
or rather from the original facts on which these propositio
rest, we can make *new* inferences, not indeed with absolut

tainty, but with a degree of conviction that is of the ost practical use. We have gained the great step of g able to make trustworthy generalizations. We con- de, for instance, not merely that John and Henry die, t that all men die.

§ 5. The above brief investigation contains, it is hoped, tolerably correct outline of the nature of the Inductive rence, as it presents itself in Material or Scientific Logic. involves the distinction drawn by Mill, and with which reader of his *System of Logic* will be familiar, between inference drawn *according* to a formula and one drawn a formula. We do in reality make our inference from data afforded by experience directly to the conclusion; a mere arrangement of convenience to do so by passing ugh the generalization. But it is one of such extreme venience, and one so necessarily forced upon us when we appealing to our own past experience or to that of others the grounds of our conclusion, that practically we find it best plan to divide the process of inference into two rts. The first part is concerned with establishing the eralization; the second (which contains the rules of ordi- y logic) determines what conclusions can be drawn from s generalization.

§ 6. We may now see our way to ascertaining the pro- ce of Probability and its relation to kindred sciences. ductive Logic gives rules for discovering such generaliza- ns as those spoken of above, and for testing their correct- s. If they are expressed in universal propositions it is part of ordinary logic to determine what inferences can made from and by them; if, on the other hand, they are pressed in proportional propositions, that is, propositions the kind described in our first chapter, they are handed er to Probability. We find, for example, that three infants out of ten die in their first four years. It belongs to Ind tion to say whether we are justified in generalizing our ob vation into the assertion, All infants die in that proporti When such a proposition is obtained, whatever may be value to be assigned to it, we recognize in it a series o familiar kind, and it is at once claimed by Probability.

In this latter case the division into two parts, the ind tive and the ratiocinative, seems decidedly more than one convenience; it is indeed imperatively necessary for clear of thought and cogency of treatment. It is true that almost every example that can be selected we shall f both of the above elements existing together and combin to determine the degree of our conviction, but when we co to examine them closely it appears to me that the grou of their cogency, the kind of conviction they produce, consequently the rules which they give rise to, are so tirely distinct that they cannot possibly be harmonized i a single consistent system.

The opinion therefore according to which certain ductive formulæ are regarded as composing a portion Probability, and which finds utterance in the Rule of S cession criticised in our last chapter, cannot, I think, maintained. It would be more correct to say, as st above, that Induction is quite distinct from Probability, co-operates in almost all its inferences. By Induction determine, for example, whether, and how far, we can sa generalize the proposition that four men in ten live to fifty-six; supposing such a proposition to be safely generali we hand it over to Probability to say what sort of infere can be deduced from it.

§ 7. So much then for the opinion which tends to reg pure Induction as a subdivision of Probability. By majority of philosophical and logical writers a widely diffe w has of course been entertained. They are mostly dised to distinguish these sciences very sharply from, not to to contrast them with, one another; the one being epted as philosophical or logical, and the other rejected mathematical. This may without offence be termed the pular prejudice against Probability.

A somewhat different view, however, must be noticed re, which, by a sort of reaction against the latter, seems m to go beyond the former; and which occasionally finds pression in the statement that all inductive reasoning of ry kind is merely a matter of Probability. Two examples this may be given.

Beginning with the older authority, there is an often oted saying by Butler at the commencement of his *Anay*, that 'probability is the very guide of life'; a saying ich seems frequently to be understood to signify that the es or principles of Probability are thus all-prevalent when are drawing conclusions in practical life. Judging by e drift of the context, indeed, this seems a fair interetation of his meaning, in so far of course as there could said to be any such thing as a science of Probability in e days. Prof. Jevons, in his *Principles of Science* 197), has expressed a somewhat similar view, of course a way more consistent with the principles of modern ence, physical and mathematical. He says, "I am conced that it is impossible to expound the methods of inction in a sound manner, without resting them on the ory of Probability. Perfect knowledge alone can give tainty, and in nature perfect knowledge would be infinite owledge, which is clearly beyond our capacities. We have, erefore, to content ourselves with partial knowledge,— owledge mingled with ignorance, producing doubt[1]."

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[1] See also Dugald Stewart (Ed. by Hamilton; VII. pp. 115—119).

§ 8. There are two senses in which this disposition t merge the two sciences into one may be understood. Usi the word Probability in its vague popular significati nothing more may be intended than to call attention to th fact, that in every case alike our conclusions are nothin more than 'probable,' that is, that they are not, and cann be, absolutely certain. This must be fully admitted, for course no one acquainted with the complexity of physi and other evidence would seriously maintain that absol ideal certainty can be attained in any branch of appli logic. Hypothetical certainty, in abstract science, may b possible, but not absolute certainty in the domain of th concrete. This has been already noticed in a former chapt where, however, it was pointed out that whatever justific tion may exist, on the subjective view of logic, for regardi this common prevalence of absence of certainty as warranti us in fusing the sciences into one, no such justification i admitted when we take the objective view.

§ 9. What may be meant, however, is that the *groun* of this absence of certainty are always of the same gene character. This argument, if admitted, would have re force, and must therefore be briefly noticed. We have se abundantly that when we say of a conclusion within th strict province of Probability, that it is not certain, all th we mean is that in some proportion of cases only will su conclusion be right, in the other cases it will be wro Now when we say, in reference to any inductive conclusi that we feel uncertain about its absolute cogency, are conscious of the same interpretation? It seems to me th we are not. It is indeed quite possible that on ultima analysis it might be proved that experience of failure i the past employment of our methods of investigation w the main cause of our present want of perfect confidence i

m. But this, as we have repeatedly insisted, does not long to the province of logical, but to that of Psycholo-al enquiry. It is surely not the case that we are, as a rule, sciously guided by such occasional or repeated instances past failure. In so far as they are at all influential, they m to do their work by infusing a vague want of confi-ce which cannot be referred to any statistical grounds for justification, at least not in a quantitative way. Part of want of confidence is derived sympathetically from those o have investigated the matter more nearly at first hand. re again, analysis might detect that a given proportion of t failures lay at the root of the distrust, but it does not w at the surface. Moreover, one reason why we cannot l perfectly certain about our inductions is, that the *mory* has to be appealed to for some of our data; and will r one assert that the only reason why we do not place olute reliance on our memory of events long past is that have been deceived in that way before?

In any other sense, therefore, than as a needful protest inst attaching too great demonstrative force to the con-sions of Inductive Logic, it seems decidedly misleading to ak of its reasonings as resting upon Probability.

§ 10. We may now see clearly the reasons for the its within which causation[1] is necessarily required, but ond which it is not needed. To be able to generalize rmula so as to extend it from the observed to the unob-ved, it is clearly essential that there should be a certain manence in the order of nature; this permanence is one t of what is implied in the term causation. If the circumstances under which men live and die remaining t same, we did not feel warranted in inferring that four m out of ten would continue to live to fifty, because in the c of those whom we had observed this proportion had hithe done so, it is clear that we should be admitting that t same antecedents need not be followed by the same c sequents. This uniformity being what the Law of Causat asserts, the truth of the law is clearly necessary to enable to obtain our generalizations: in other words, it is necess for the Inductive part of the process. But it seems to equally clear that causation is not necessary for that part the process which belongs to Probability. Provided o that the truth of our generalizations is secured to us, in way just mentioned, what does it matter to us whether not the individual members are subject to causation? it is not in reality about these individuals that we m inferences. As this last point has been already fully trea in Chapter VI., any further allusion to it need not be made h

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[1] Required that is for purposes of al inference within the limits of ability; it is not intended to im-any doubts as to its actual universal prevalence, or its all-importance for scientific purposes. The subject is more fully discussed in a future chapter.

§ 11. The above description, or rather indication, of process of obtaining these generalizations must suffice for present. Let us now turn and consider the means by wh we are practically to make use of them when they are tained. The point which we had reached in the course of investigations entered into in the sixth and seventh chap was this:—Given a series of a certain kind, we could d inferences about the members which composed it; inferen that is, of a peculiar kind, the value and meaning of wh were fully discussed in their proper place.

We must now shift our point of view a little; instead starting, as in the former chapters, with a determinate se supposed to be given to us, let us assume that the indivi only is given, and that the work is imposed upon us of ing out the appropriate series. How are we to set about k? In the former case our data were of this kind:— ght out of ten men, aged fifty, will live eleven years more, d we ascertained in what sense, and with what certainty, could infer that, say, John Smith, aged fifty, would live sixty-one.

§ **12.** Let us then suppose, instead, that John Smith sents himself, how should we in this case set about obning a series for him? In other words, how should we llect the appropriate statistics? It should be borne in nd that when we are attempting to make real inferences out things as yet unknown, it is in this form that the blem will practically present itself.

At first sight the answer to this question may seem to be tained by a very simple process, viz. by counting how ny men of the age of John Smith, respectively do and do live for eleven years. In reality however the process is from being so simple as it appears. For it must be rembered that each individual thing has not one distinct d appropriate class or group, to which, and to which alone, properly belongs. We may indeed be practically in the bit of considering it under such a single aspect, and it may erefore seem to us more familiar when it occupies a place one series rather than in another; but such a practice is erely customary on our part, not obligatory. It is obvious at every individual thing or event has an indefinite mber of properties or attributes observable in it, and ight therefore be considered as belonging to an indefinite mber of different classes of things. By belonging to any e class it of course becomes at the same time a member of l the higher classes, the genera, of which that class was a ecies. But, moreover, by virtue of each accidental attrite which it possesses, it becomes a member of a class tersecting, so to say, some of the other classes. John Smith

is a consumptive man say, and a native of a northern clin
Being a man he is of course included in the class of
tebrates, also in that of animals, as well as in any hi
such classes that there may be. The property of being
sumptive refers him to another class, narrower than an
the above; whilst that of being born in a northern clir
refers him to a new and distinct class, not conterminous
any of the rest, for there are things born in the north w
are not men.

§ 13. When therefore John Smith presents himsel
our notice without, so to say, any particular label attache
him informing us under which of his various aspects he
be viewed, the process of thus referring him to a class
comes to a great extent arbitrary. If he had been indic
to us by a general name, that, of course, would have
some clue; for the name having a determinate connota
would specify at any rate a fixed group of attributes wi
which our selection was to be confined. But names
attributes being connected together, we are here supp
to be just as much in ignorance what name he is t
called by, as what group out of all his innumerable attrib
is to be taken account of; for to tell us one of these th
would be precisely the same in effect as to tell us the o
In saying that it is thus arbitrary under which class h
placed, we mean, of course, that there are no logical grou
of decision; the selection must be determined by some
traneous considerations. Mere inspection of the indivi
would simply show us that he could equally be refe
to an indefinite number of classes, but would in itself
no inducement to prefer, for our special purpose, one of t
classes to another.

This variety of classes to which the individual ma
referred owing to his possession of a multiplicity of a

es, has an important bearing on the process of inference ich was indicated in the earlier sections of this chapter, d which we must now examine in more special reference our particular subject.

§ 14. It will serve to bring out more clearly the nature some of those peculiarities of the step which we are now out to take in the case of Probability, if we first examine e form which the corresponding step assumes in the case ordinary Logic. Suppose then that we wished to ascertain hether a certain John Smith, a man of thirty, who is ongst other things a resident in India, and distinctly ected with cancer, will continue to survive there for nty years longer. The terms in which the man is thus roduced to us refer him to different classes in the way eady indicated. Corresponding to these classes there will a number of propositions which have been obtained by evious observations and inductions, and which we may erefore assume to be available and ready at hand when e want to make use of them. Let us conceive them to such as these following:—Some men live to fifty; some dian residents live to fifty; no man suffering thus from ncer lives for five years. From the first and second of these emises nothing whatever can be inferred, for they are both[1] rticular propositions, and therefore lead to no conclusion this case. The third answers our enquiry decisively.

To the logical reader it will hardly be necessary to point t that the process here under consideration is that of ding middle terms which shall serve to connect the bject and predicate of our conclusion. This subject and edicate in the case in question, are the individual before us and his death within the stated period. Regard themselves there is nothing in common between then therefore no link by which they may be connected o connected with each other. The various classes referred to are a set of such middle terms, and the pr tions belonging to them are a corresponding set of premises. By the help of any one of them we are en under suitable circumstances, to connect together the s and predicate of the conclusion, that is, to infer wheth man will or will not live twenty years.

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[1] As particular propositions they e both of course identical in form. e fact that the 'some' in the former corresponds to a larger proportion than in the latter, is a distinction alien to pure Logic.

§ 15. Now in the performance of such a logical p there are two considerations to which the reader's att must for a moment be directed. They are simple enou this case, but will need careful explanation in the corres ing case in Probability. In the first place, it is clea whenever we can make any inference at all, we can with absolute certainty. Logic, within its own d knows nothing of hesitation or doubt. If the middle is appropriate it serves to connect the extremes in s way as to preclude all uncertainty about the concl if it is not, there is so far an end of the matter: no c sion can be drawn, and we are therefore left where we Assuming our premises to be correct, we either kno conclusion for certain, or we know nothing whatever it. In the second place, it should be noticed that n the possible alternatives in the shape of such major pr as those given above can ever contradict any of the or be at all inconsistent with them. Regarded as is propositions, there is of course nothing to secure suc mony; they have very different predicates, and may quite out of each other's reach for either support or o tion. But by means of the other premise they are i case brought into relation with one another, and the g

erests of truth and consistency prevent them therefore
m contradicting one another. As isolated propositions
might have been the case that all men live to fifty, and
at no Indian residents do so, but having recognised that
me men are residents in India, we see at once that these
mises are inconsistent, and therefore that one or other
them must be rejected. In all applied logic this necessity
avoiding self-contradiction is so obvious and imperious
at no one would think it necessary to lay down the formal
stulate that all such possible major premises are to be
tually consistent. To suppose that this postulate is not
mplied with, would be in effect to make two or more con-
dictory assumptions about matters of fact.

§ 16. But now observe the difference when we attempt
take the corresponding step in Probability. For ordinary
positions, universal or particular, substitute statistical
positions of what we have been in the habit of calling
e 'proportional' kind. In other words, instead of asking
ether the man will live for twenty years, let us ask whether
will live for one year? We shall be unable to find any
iversal propositions which will cover the case, but we may
thout difficulty obtain an abundance of appropriate pro-
rtional ones. They will be of the following description:—
f men aged 30, 98 in 100 live another year; of residents in
dia a smaller proportion survive, let us for example say
in 100; of men suffering from cancer a smaller proportion
ll, let us say 20 in 100.

Now in both of the respects to which attention has just
en drawn, propositions of this kind offer a marked con-
st with those last considered. In the first place, they do
ot, like ordinary propositions, either assert unequivocally
s or no, or else refuse to open their lips; but they give
stead a sort of qualified or hesitating answer concerning

the individuals included in them. This is of course not
more than the familiar characteristic of what may be ca
'probability propositions.' But it leads up to, and in
renders possible, the second and more important po
viz. that these various answers, though they cannot dire
and formally contradict each other (this their nature as
portional propositions, will not as a rule permit), may ye
a way which will now have to be pointed out, be found t
more or less in conflict with each other.

Hence it follows that in the attempt to draw a conclu
from premises of the kind in question, we may be pl
in a position of some perplexity; but it is a perple
which may present itself in two forms, a mild and an ag
vated form. We will notice them in turn.

§ 17. The mild form occurs when the different cl
to which the individual case may be appropriately refe
are successively included one within another; for here
sets of statistics, though leading to different results,
not often be found to be very seriously at variance
one another. All that comes of it is that as we ascend in
scale by appealing to higher and higher genera, the
tistics grow continually less appropriate to the partic
case in point, and such information therefore as they a
becomes gradually less explicit and accurate.

The question that we originally wanted to deter
be it remembered, is whether John Smith will die wi
one year. But all knowledge of this fact being unat
able, owing to the absence of suitable inductions, we
justified (with the explanation, and under the restric
mentioned in Chap. VI.), in substituting, as the only ava
equivalent for such individual knowledge, the answer t
following statistical enquiry, What proportion of men i
circumstances die?

§ 18. But then at once there begins to arise some doubt d ambiguity as to what exactly is to be understood by his cumstances. We may know very well what these circumnces are in themselves, and yet be in perplexity as to w many of them we ought to take into account when deavouring to estimate his fate. We might conceivably, a beginning, choose to confine our attention to those operties only which he has in common with all animals. so, and statistics on the subject were attainable, they uld presumably be of some such character as this, Ninety-ne animals out of a hundred die within a year. Unusual as ch a reference would be, we should, logically speaking, be ing nothing more than taking a wider class than the one were accustomed to. Similarly we might, if we pleased, ke our stand at the class of vertebrates, or at that of mmalia, if zoologists were able to give us the requisite formation. Of course we reject these wide classes and efer a narrower one. If asked why we reject them, the tural answer is that they are so general, and resemble the rticular case before us in so few points, that we should be ceedingly likely to go astray in trusting to them. Though curacy cannot be insured, we may at least avoid any needs exaggeration of the relative number and magnitude of r errors.

§ 19. The above answer is quite valid; but whilst cauning us against appealing to too wide a class, it seems to ggest that we cannot go wrong in the opposite direction, t is in taking too narrow a class. And yet we do avoid y such extremes. John Smith is not only an Englishman; may also be a native of such a part of England, be living such a Presidency, and so on. An indefinite number of ch additional characteristics might be brought out into otice, many of which at any rate have some bearing upon the question of vitality. Why do we reject any conside of these narrower classes? We do reject them, but it what may be termed a practical rather than a theo reason. As was explained in the first chapters, it is ess that our series should contain a considerable number of if they are to be of any service to us. Now many attributes of any individual are so rare that to take into account would be at variance with the fundar assumption of our science, viz. that we are properly con only with the averages of large numbers. The more and minute our statistics the better, provided only th can get enough of them, and so make up the requisite number of instances. This is, however, impossible in cases. We are therefore obliged to neglect one att after another, and so to enlarge the contents of our cla the avowed risk of somewhat increased variety and u ability in the members of it, for at each step of this ki diverge more and more from the sort of instances th really want. We continue to do so, until we no longe more in quantity than we lose in quality. We finall our stand at the point where we first obtain statistics from a sufficiently large range of observation to secu requisite degree of stability and uniformity.

§ 20. In such an example as the one just men where one of the successive classes—man—is a well-c natural kind or species, there is such a complete br each direction at this point, that every one is prom take his stand here. On the one hand, no enquirer ever think of introducing any reference to the higher with fewer attributes, such as animal or organized and on the other hand, the inferior classes, created taking notice of his employment or place of residen do not as a rule differ sufficiently in their charact m the class *man* to make it worth our while to attend them.

Now and then indeed these characteristics do rise into portance, and whenever this is the case we concentrate r attention upon the class to which they correspond, that , the class which is marked off by their presence. Thus, r instance, the quality of consumptiveness separates any e off so widely from the majority of his fellow-men in all estions pertaining to mortality, that statistics about the ves of consumptive men differ materially from those which fer to men in general. And we see the result; if a con- mptive man can effect an insurance at all, he must do it r a much higher premium, calculated upon his special rcumstances. In other words, the attribute is sufficiently portant to mark off a fresh class or series. So with in- rance against accident. It is not indeed attempted to ake a special rate of insurance for the members of each eparate trade, but the differences of risk to which they are able oblige us to take such facts to some degree into ccount. Hence, trades are roughly divided into two or hree classes, such as the ordinary, the hazardous, and the xtra-hazardous, each having to pay its own rate of premium.

§ 21. Where one or other of the classes thus corresponds o natural kinds, or involves distinctions of co-ordinate im- ortance with those of natural kinds, the process is not ifficult; there is almost always some one of these classes hich is so universally recognised to be the appropriate one, hat most persons are quite unaware of there being any ecessity for a process of selection. Except in the cases here a man has a sickly constitution, or follows a dangerous mployment, we seldom have occasion to collect statistics for im from any class but that of men in general of his age in e country.

When, however, these successive classes are not re
marked out for us by nature, and thence arranged in ea
distinguishable groups, the process is more obviously a
trary. Suppose we were considering the chance of a m
house being burnt down, with what collection of attribu
should we rest content in this instance? Should we incl
all kinds of buildings, or only dwelling-houses, or conf
ourselves to those where there is much wood, or those wh
have stoves? All these attributes, and a multitude of oth
may be present, and, if so, they are all circumstances wh
help to modify our judgment. We must be guided here
the statistics which we happen to be able to obtain
sufficient numbers. Here again, rough distinctions of t
kind are practically drawn in Insurance Offices, by divid
risks into ordinary, hazardous, and extra-hazardous.
examine our case, refer it to one or other of these clas
and then form our judgment upon its prospects by the s
tistics appropriate to its class.

§ 22. So much for what may be called the mild form
which the ambiguity occurs; but there is an aggravated f
in which it may show itself, and which at first sight se
to place us in far greater perplexity.

Suppose that the different classes mentioned above
not included successively one within the other. We n
then be quite at a loss which of the statistical tables
employ. Let us assume, for example, that nine out of
Englishmen are injured by residence in Madeira, but t
nine out of ten consumptive persons are benefited by suc
residence. These statistics, though fanciful, are conceiva
and perfectly compatible. John Smith is a consump
Englishman; are we to recommend a visit to Madeira in
case or not? In other words, what inferences are we
draw about the probability of his death? Both of the

ical tables apply to his case, but they would lead us to ctly contradictory conclusions. This does not mean, of rse, contradictory precisely in the logical sense of that rd, for one of these propositions does not assert that an nt must happen and the other deny that it must; but tradictory in the sense that one would cause us in some siderable degree to believe what the other would cause us some considerable degree to disbelieve. This refers, of rse, to the individual events; the statistics are by suppo- ion in no degree contradictory. Without further data, refore, we can come to no decision.

§ 23. Practically, of course, if we were forced to a deci- n with only these data before us, we should make our ice by the consideration that the state of a man's lungs s probably more to do with his health than the place of i birth has; that is, we should conclude that the duration life of consumptive Englishmen corresponds much more sely with that of consumptive persons in general than th that of their healthy countrymen. But this is, of urse, to import empirical considerations into the question. e data, as they are given to us, and if we confine our- ves to them, leave us in absolute uncertainty upon the int. It may be that the consumptive Englishmen almost die when transported into the other climate; it may be at they almost all recover. If they die, this is in obvious cordance with the first set of statistics; it will be found in cordance with the second set through the fact of the reign consumptives profiting by the change of climate in ore than what might be termed their due proportion. similar explanation will apply to the other alternative, to the supposition that the consumptive Englishmen ostly recover. The problem is, therefore, left absolutely determinate, for we cannot here appeal to any general rule

so simple and so obviously applicable as that which, i
former case, recommended us always to prefer the m
special statistics, when sufficiently extensive, to those w
are wider and more general. We have no means he
knowing whether one set is more special than the other.

And in this no difficulty can be found, so long as
confine ourselves to a just view of the subject. Let
again recall to the reader's mind what our present posi
is; we have substituted for knowledge of the indivi
(finding that unattainable) a knowledge of what occur
the average of similar cases. This step had to be taken
moment the problem was handed over to Probability.
the conception of similarity in the cases introduces us t
perplexity; we manage indeed to evade it in many
stances, but here it is inevitably forced upon our no
There are here two aspects of this similarity, and th
introduce us to two distinct averages. Two assertions
made as to what happens in the long run, and both of th
assertions, by supposition, are verified. Of their truth th
need be no doubt, for both were supposed to be obtai
from experience.

§ 24. It may perhaps be supposed that such an examp
as this is a *reductio ad absurdum* of the principle upon wh
Life and other Insurances are founded. But a momen
consideration will show that this is quite a mistake,
that the principle of insurance is just as applicable
examples of this kind as to any other. An office need f
no difficulty in the case supposed. They *might* (for a reas
to be mentioned presently, they probably *would* not) insu
the individual without inconsistency at a rate determi
by either average. They might say to him, "You are
Englishman. Out of the multitude of English who come
us nine in ten die if they go to Madeira. We will insu

ou at a rate assigned by these statistics, knowing that in
e long run all will come right so far as we are concerned.
ou are also consumptive, it is true, and we do not know
hat proportion of the English are consumptive, nor what
oportion of English consumptives die in Madeira. But
is does not really matter for our purpose. The formula,
ne in ten die, is in reality calculated by taking into account
ese unknown proportions; for, though we do not know
em in themselves, statistics tell us all that we care to
ow about their results. In other words, whatever un-
own elements may exist, must, in regard to all the effects
hich they can produce, have been already taken into
count, so that our ignorance about them cannot in the
st degree invalidate such conclusions as we are able to
aw. And this is sufficient for our purpose." But precisely
e same language might be held to him if he presented
imself as a consumptive man; that is to say, the office
uld safely carry on its proceedings upon either alternative.

This would, of course, be a very imperfect state for the
atter to be left in. The only rational plan would be to
olate the case of consumptive Englishmen, so as to make
separate calculation for their circumstances. This cal-
ulation would then at once supersede all other tables so
r as they were concerned; for though, *in the end,* it could
ot arrogate to itself any superiority over the others, it
ould in the mean time be marked by fewer and slighter
berrations from the truth.

§ 25. The real reason why the Insurance office could
ot long work on the above terms is of a very different
ind from that which some readers might contemplate, and
elongs to a class of considerations which have been much
eglected in the attempts to construct sciences of the dif-
erent branches of human conduct. It is nothing else than

that annoying contingency to which prophets since the t of Jonah have been subject, of uttering *suicidal* prophec of publishing conclusions which are perfectly certain w every condition and cause but one have been taken i account, that one being the effect of the prophecy it upon those to whom it refers.

In our example above, the office (in so far as the pa cular cases in Madeira are concerned) would get on very v until the consumptive Englishmen in question found what much better terms they could make by announc themselves as consumptives, and paying the premium propriate to that class, instead of announcing themselves Englishmen. But if they did this they would of course disturbing the statistics. The tables were based upon t assumption that a certain fixed proportion (it does matter what proportion) of the English lives would conti to be consumptive lives, which, under the supposed circu stances, would probably soon cease to be true. When i said that nine Englishmen out of ten die in Madeira, it meant that of those who come to the office, as the phrase at random, or in their fair proportions, nine-tenths die. T consumptives are supposed to go there just like red-hai men, or poets, or any other special class. Or they might in any proportions greater or less than those of other class so long as they adhered to the same proportion througho The tables are then calculated on the continuance of t state of things; the practical contradiction is in supposi such a state of things to continue after the people had o had a look at the tables. If we merely make the assum tion that the publication of these tables made no such alte tion in the conduct of those to whom it referred, no hitch this kind need occur.

§ 26. The assumptions here made, as has been said, in any way contradictory, but they need some explana- n. It will readily be seen that, taken together, they are onsistent with the supposition that each of these classes is mogeneous, that is, that the statistical proportions which ld of the whole of either of them will also hold of any rtion of them which we may take. There are certain ividuals (viz. the consumptive Englishmen) who belong each class, and of course the two different sets of statistics nnot both be true of them taken by themselves. They ght coincide in their characteristics with either class, but t with both; probably in most practical cases they will ncide with neither, but be of a somewhat intermediate aracter. Now when it is said of any such heterogeneous dy that, say, nine-tenths die, what is meant (or rather plied) is that the class might be broken up into smaller bdivisions of a more homogeneous character, in some of ich, of course, more than nine-tenths die, whilst in others s, the differences depending upon their character, consti- tion, profession, &c.; the number of such divisions and the ount of their divergence from one another being perhaps ry considerable.

Now when we speak of either class as a whole and say at nine-tenths die, the most natural and soundest mean- g is that that would be the proportion if all without ception went abroad, or (what comes to the same thing) if ch of these various subdivisions was represented in fair oportion to its numbers. Or it might only be meant that ey go in some other proportion, depending upon their tes, pursuits, and so on. But whatever meaning be adopted e condition is necessary, viz. that the proportion of each ss that went at the time the statistics were drawn up ust be adhered to throughout. When the class is homo- neous this is not needed, but when it is heterogeneous the statistics would be interfered with unless this condition w secured.

We are here supposed to have two sets of statistics, o for the English and one for the consumptives, so that t consumptive English are in a sense counted twice over. their mortality is of an intermediate amount, therefore, th serve to keep down the mortality of one class and to ke up that of the other. If the statistics are supposed to exhaustive, by referring to the whole of each class, it follo that actually the same individuals must be counted ea time; but if representatives only of each class are taken, t same individuals need not be inserted in each set of tables

§ 27. When therefore they come to insure (our remar are still confined to our supposed Madeira case), we ha some English consumptives counted as English, and payi the high rate; and others counted as consumptives and p ing the low rate. Logically indeed we may suppose them entered in each class, and paying therefore each rate. Wh we have said above is that any individual may be conceiv to present himself for either of these classes. Conceive th some one else pays his premium for him, so that it is matter of indifference to him personally at which rate h insures, and there is nothing to prevent some of the class (for that matter all) going to one class, and others (or again) going to the other class.

So long therefore as we make the logically possib though practically absurd supposition that some men w continue to pay a higher rate than they need, there is n thing to prevent the English consumptives (some or all) fro insuring in each category and paying its appropriate pr mium. As soon as they gave any thought to the matter, course they would, in the case supposed, all prefer to ins as consumptives. But their doing this would disturb each of statistics. The English mortality in Madeira would instantly become heavier, so far as the Insurance company was concerned, by the loss of all their best lives; whilst the consumptive statistics (unless *all* the English consumptives had already been taken for insurance) would be in the same way deteriorated[1]. A slight readjustment therefore of each scale of insurance would then be needed; this is the disturbance mentioned just above. It must be clearly understood, however, that it is not our original statistics which have proved to be inconsistent, but simply that there were practical obstacles to carrying out a system of insurance upon them.

§ 28. Examples subject to the difficulty now under consideration will doubtless seem perplexing to the student unacquainted with the subject. They are difficult to reconcile with any other view of the science than that insisted on throughout this Essay, viz. that we are only concerned with averages. It will perhaps be urged that there are two different values of the man's life in these cases, and that they cannot both be true. Why not? The 'value' of his life is simply the number of years to which men in his circumstances do, on the average, attain; we have the man set before us under two different circumstances; what wonder, therefore, that these should offer different averages? In such an objection it is forgotten that we have had to substitute for the unattainable result about the individual, the really attainable result about a set of men as much like him as possible. The difficulty and apparent contradiction only arise when people will try to find some justification for their belief in the individual case. What can we possibly conclude, it may be asked, about this particular man Jo Smith's prospects when we are thus offered two diffe values for his life? Nothing whatever, it must be repli nor could we in reality draw a conclusion, be it remember in the former case, when we were practically confined to set of statistics. There also we had what we called 'value' of his life, and since we only knew of one such val we came to regard it as in some sense appropriate to him an individual. Here, on the other hand, we have two valu belonging to different series, and as these values are re different it may be complained that they are discordant, such a complaint can only be made when we do what have no right to do, viz. assign a value to the individ which shall admit of individual justification.

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[1] The reason is obvious. The healthiest English lives in Madeira (viz. the consumptive ones) have now ceased to be reckoned as English; whereas the worst consumptive lives there (viz. the English) are now increased in relative numbers.

§ 29. Is it then perfectly arbitrary what series or cl of instances we select by which to judge? By no means; has been stated repeatedly that in choosing a series, we m seek for one the members of which shall resemble our in vidual in as many of his attributes as possible, subject on to the restriction that it must be a sufficiently extensi series. What is meant is, that in the above case, where have two series, we cannot fairly call them contradictory; only valid charge is one of incompleteness or insufficiency their purpose, a charge which applies in exactly the sa sense, be it remembered, to all statistics which compr genera unnecessarily wider than the species with which are concerned. The only difference between the two diffe ent classes of cases is, that in the one instance we are on path which we know will lead at the last, through ma errors, towards the truth (in the sense in which truth can attained here), and we took it for want of a better. In t other instance we have two such paths, perfectly differe paths, either of which however will lead us towards the tru before. Contradiction can only seem to arise when it is tempted to justify each separate step on our paths, as well their ultimate tendency.

Still it cannot be denied that these objections are a rious drawback to the completeness and validity of any ticipations which are merely founded upon statistical fre- ency, at any rate in an early stage of experience, when t few statistics have been collected. Such knowledge as obability can give is not in any individual case of a high der, being subject to the characteristic infirmity of re- ated error; but even when measured by its own stand- d it commences at a very low stage of proficiency. The rors are then relatively very numerous and large compared ith what they may ultimately be reduced to.

§ 30. Here as elsewhere there is a continuous process specialization going on. The needs of a gradually widen- g experience are perpetually calling upon us to subdivide lasses which are found to be too heterogeneous. Sometimes he only complaint that has to be made is that the class to hich we are obliged to refer is found to be somewhat too road to suit our purpose, and that it might be subdivided ith convenience. This is the case, as has been shown above, hen an Insurance office finds that its increasing business akes it possible and desirable to separate off the men who llow some particular trades from the rest of their fellow- untrymen. Similarly in every other department in which tatistics are made use of. This increased demand for speci- cness leads, in fact, as naturally in this direction, as does he progress of civilization to the subdivision of trades in y town or country. So in reference to the other kind of rplexity mentioned above. Nothing is more common in hose sciences or practical arts, in which deduction is but ttle available, and where in consequence our knowledge is for the most part of the empirical kind, than to meet wi suggestions which point more or less directly in contra directions. Whenever some new substance is discovered brought into more general use, those who have to deal wi it must be familiar with such a state of things. The medic man who has to employ a new drug may often find him self confronted by the two distinct recommendations, that the one hand it should be employed for certain diseases, a that on the other hand it should not be tried on certain co stitutions. A man with such a constitution, but sufferi from such a disease, presents himself; which recommend tion is the doctor to follow? He feels at once obliged set to work to collect narrower and more special statisti in order to escape from such an ambiguity.

§ 31. In this and a multitude of analogous cas afforded by the more practical arts it is not of course nec sary that numerical data should be quoted and appeal to; it is sufficient that the judgment is more or less co sciously determined by them. All that is necessary to ma the examples appropriate is that we should admit that i their case statistical data are our ultimate appeal in t present state of knowledge. Of course if the empiric laws can be resolved into their component causes we m appeal to direct deduction, and in this case the emplo ment of statistics, and consequently the use of the theory Probability, may be superseded.

In this direction therefore, as time proceeds, the advan of statistical refinement by the incessant subdivision of clas to meet the developing wants of man is plain enough. B if we glance backwards to a more primitive stage, we sh soon see in what a very imperfect state the operation co mences. At this early stage, however, Probability and I duction are so closely connected together as to be very apt merged into one, or at any rate to have their functions founded.

§ 32. Since the generalization of our statistics is found to ong to Induction, this process of generalization may be arded as prior to, or at least independent of, Probability. have, moreover, already discussed (in Chapter VI.) the corresponding to what are termed immediate inferences, (in Chapter VII.) that corresponding to syllogistic infer- ces. Our present position therefore is that in which we y consider ourselves in possession of any number of gene- zations, but wish to employ them so as to make inferences ut a given individual; just as in one department of mmon logic we are engaged in finding middle terms to ablish the desired conclusion. In this latter case the cess is found to be extremely simple, no accumulation of erent middle terms being able to lead to any real ambi- ty or contradiction. In Probability, however, the case is erent. Here, if we attempt to draw inferences about the ividual case before us, as often is attempted—in the Rule Succession for example—we shall encounter the full force this ambiguity and contradiction. Treat the question, ever, fairly, and all difficulty disappears. Our inference lly is not about the individuals as individuals, but about ies or successions of them. We wished to know whether hn Smith will die within the year; this, however, cannot known. But John Smith, by the possession of many ributes, belongs to many different series. The multi- city of middle terms, therefore, is what ought to be pected. We *can* know whether a succession of men, resi- nts in India, consumptives, &c. die within a year. We y make our selection, therefore, amongst these, and in the g run the belief and consequent conduct of ourselves and er persons (as described in Chapter VI.) will become capable of justification. With regard to choosing o these series rather than another, we have two opp principles of guidance. On the one hand, the more sp the series the better; for, though not more right in the we shall thus be more nearly right all along. But, o other hand, if we try to make the series too special, we generally meet the practical objection arising from in cient statistics.

CHAPTER X.

CHANCE AS OPPOSED TO CAUSATION AND DESIGN.

1. THE remarks in the previous chapter will have served o clear the way for an enquiry which probably excites more opular interest than any other within the range of our subject, z. the determination whether such and such events are to e attributed to Chance on the one hand, or to Causation or Design on the other. As the principal difficulty seems to rise from the ambiguity with which the problem is generally onceived and stated, owing to the extreme generality of the onceptions involved, it becomes necessary to distinguish learly between the several distinct issues which are apt to e involved.

I. There is, to begin with, a very old objection, founded n the assumption which our science is supposed to make of he existence of *Chance*. The objection against chance is f course many centuries older than the Theory of Pro-ability; and as it seems a nearly obsolete objection at the resent day we need not pause long for its consideration. f we spelt the word with a capital C, and maintained that was representative of some distinct creative or adminis-ative agency, we should presumably be guilty of some form f Manicheism. But the only rational meaning of the objection would appear to be that the principles of the scie compel us to assume that events (some events, only, that happen without causes, and are thereby removed from customary control of the Deity. As repeatedly pointed already this is altogether a mistake. The science of bability makes no assumption whatever about the way which events are brought about, whether by causation without it. All that we undertake to do is to establish explain a body of rules which are applicable to classes cases in which we do not or cannot make inferences ab the individuals. The objection therefore must be so what differently stated, and appears finally to reduce it to this;—that the assumptions upon which the science Probability rests, are not inconsistent with a disbelief causation within certain limits; causation being of cou understood simply in the sense of regular sequence. stated the objection seems perfectly valid, or rather the f on which it is based must be admitted; though what c nection there would be between such lack of causation a absence of Divine superintendence I quite fail to see.

As this Theological objection died away the men physical science, and those who sympathized with the began to enforce the same protest; and similar cautions still to be found from time to time in modern treatis Hume, for instance, in his short essay on *Probability*, co mences with the remark, "though there be no such thing chance in the world, our ignorance of the real cause of a event has the same influence on the understanding, & De Morgan indeed goes so far as to declare that " foundations of the theory of Probability have ceased to ex in the mind that has formed the conception," "that anythi ever did happen or will happen without some particu reason why it should have been precisely what it was a

ything else[1].” Similar remarks might be quoted from
e and others.

2. In the particular form of the controversy above
d to, and which is mostly found in the region of the
l and physical sciences, the contention that chance
usation are irreconcileable occupies rather a defensive
n; the main fact insisted on being that, whenever in
subjects we may happen to be ignorant of the details
ve no warrant for assuming as a consequence that the
are uncaused. But this supposed irreconcileability
etimes urged in a much more aggressive spirit in refer-
o social enquiries. Here the attempt is often made to
causation in the details, from the known and admitted
rity in the averages. A considerable amount of con-
y was excited some years ago upon this topic, in great
riginated by the vigorous and outspoken support of the
itarian side by Buckle in his *History of Civilization.*

should be remarked that in these cases the attempt is
imes made as it were to startle the reader into acqui-
e by the singularity of the examples chosen. Instances
lected which, though they possess no greater logical
are, if one may so express it, emotionally more effective.

reader of Buckle's History, for instance, will remember
ess which he laid upon the observed fact, that the number
cides in London remains about the same, year by year;
e may remember also the sort of panic with which the
lgation of this fact was accompanied in many quarters.
the way in which Laplace notices that the number of
cted letters annually sent to the Post Office remains
the same, and the comments of Dugald Stewart upon
articular uniformity, seem to imply that they regarded

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[1] *Essay on Probabilities*, p. 114.

this instance as more remarkable than many analogous o taken from other quarters.

That there is a certain foundation of truth in the reas ings in support of which the above examples are advanc cannot be denied, but their authors appear to me very m to overrate the sort of opposition that exists between t theory of Chances and the doctrine of Causation. As rega first that wider conception of order or regularity which have termed uniformity, anything which might be cal objective chance would certainly be at variance with this one respect. In Probability ultimate regularity is alw postulated; in tossing a die, if not merely the individ throws were uncertain in their results, but even the avera also, owing to the nature of the die, or the number of t marks upon it, being arbitrarily interfered with, of course kind of science would attempt to take any account of it.

§ 3. So much must undoubtedly be granted; but mu the same admission be made as regards the succession of t individual events? Can causation, in the sense of invariab succession (for we are here shifting on to this narrow ground), be denied, not indeed without suspicion of scienti heterodoxy, but at any rate without throwing uncertain upon the foundations of Probability? De Morgan, as have seen, strongly maintains that this cannot be so. I fi myself unable to agree with him here, but this disagreeme springs not so much from differences of detail, as from th of the point of view in which we regard the science. always appears to incline to the opinion that the indi dual judgment in probability is to admit of justificatio that when we say, for instance, that the odds in favour some event are three to two, that we can explain and justi our statement without any necessary reference to a series class of such events. It is not easy to see how this can me in any case, but the obstacles would doubtless be eater even than they are, if knowledge of the individual ent were not merely unattained, but, owing to the absence any causal connection, essentially unattainable. On the eory adopted in this work we simply postulate ignorance the details, but it is not regarded as of any importance what sort of grounds this ignorance is based. It may that knowledge is out of the question from the nature the case, the causative link, so to say, being missing. It y be that such links are known to exist, but that either cannot ascertain them, or should find it troublesome to so. It is the fact of this ignorance that makes us appeal the theory of Probability, the grounds of it are of no portance.

§ 4. On the view here adopted we are concerned only th averages, or with the single event as deduced from an erage and conceived to form one of a series. We start th the assumption, grounded on experience, that there is iformity in this average, and, so long as this is secured to , we can afford to be perfectly indifferent to the fate, as gards causation, of the individuals which compose the erage. The question then assumes the following form:— this assumption, of average regularity in the aggregate, onsistent with the admission of what may be termed useless irregularity in the details? It does not seem to me at it would be at all easy to prove that this is so. As atter of fact the two beliefs have constantly co-existed in e same minds. This may not count for much, but it sug- sts that if there be a contradiction between them it is by means palpable and obvious. Millions, for instance, have ieved in the general uniformity of the seasons taken one h another, who certainly did not believe in, and would y likely have been ready distinctly to deny, the existence

of necessary sequences in the various phenomena which co
pose what we call a season. So with cards and dice; alm
every gambler must have recognized that judgment a
foresight are of use in the long run, but writers on cha
seem to think that gamblers need a good deal of reasoning
convince them that each separate throw is in its nature
sentially predictable.

§ 5. In its application to moral and social subje
what gives this controversy its main interest is its real
supposed bearing upon the vexed question of the freed
of the will; for in this region Causation, and Fatalism
Necessitarianism, are regarded as one and the same thing.

Here, as in the last case, that wide and somewhat va
kind of regularity that we have called Uniformity, must
admitted as a notorious fact. Statistics have put it ou
the power of any reasonably informed person to feel
hesitation upon this point. Some idea has already b
gained, in the earlier chapters, of the nature and amo
of the evidence which might be furnished of this fact,
any quantity more might be supplied from the works
professed writers upon the subject. If, therefore, Free-will
so interpreted as to imply such essential irregularity as de
prediction both in the average, and also in the single c
then the negation of free-will follows, not as a remote logi
consequence, but as an obvious inference from indisput
facts of experience.

Few persons, however, would go so far as to interpre
in this sense. All that troubles them is the fear that so
how this general regularity may be found to carry wit
causation, certainly in the sense of regular invariable
quence, and probably also with the further associatio
compulsion. Rejecting the latter association as utt
unphilosophical, I cannot even see that the former ca

uence can be admitted as really proved, though it doubtless ains some confirmation from this source.

§ 6. The nature of the argument against free-will, rawn from statistics, at least in the form in which it is very ommonly expressed, seems to me exceedingly defective. he antecedents and consequents, in the case of our voli-ions, must clearly be supposed to be very nearly *immediately* n succession, if anything approaching to causation is to be stablished: whereas in statistical enquiries the data are ften widely separate, if indeed they do not apply merely o single groups of actions or results. For instance, in the ase of the misdirected letters, what it is attempted to prove that each writer was so much the 'victim of circumstances' o use a common but misleading expression) that he could ot have done otherwise than he did under his circumstances. ut really no accumulation of figures to prove that the umber of such letters remains the same year by year, can ave much bearing upon this doctrine, even though they ere accompanied by corresponding figures which should onnect the forgetfulness thus indicated with some other aracteristics in the writers. So with the number of icides. If 250 people do, or lately did, annually put an d to themselves in London, the fact, as it thus stands by elf, may be one of importance to the philanthropist and atesman, but it needs bringing into much closer relation ith psychological elements if it is to convince us that the ctions of men are always instances of inflexible order. In ct, instead of having secured our A and B here in closest timacy of succession to one another,—to employ the symolic notation commonly used in works on Inductive Logic illustrate the causal connection,—we find them sepated by a considerable interval; often indeed we merely ave an A or a B by itself.

§ 7. Again, another deficiency in such reasoning see
to be the laying undue weight upon the mere regularity
persistency of the statistics. These may lead to very i
portant results, but they are not exactly what is want
for the purpose of proving anything against the freedo
of the will; it is not indeed easy to see what connecti
this has with such facts as that the annual number of the
or of suicides remains at pretty nearly the same figu
Statistical uniformity seems to me to establish nothing el
at least directly, in the case of human actions, than it do
in that of physical characteristics. Take but one instan
that of the misdirected letters. We were already aw
that the height, weight, chest measurement, and so on,
a large number of persons preserved a tolerably regul
average amidst innumerable deflections, and we were p
pared by analogy to anticipate the same regularity in th
mental characteristics. All that we gain, by counting t
numbers of letters which are posted without addresses,
a certain amount of direct evidence that this is the ca
Just as observations of the former kind had already sho
that statistics of the strength and stature of the hum
body grouped themselves about a mean, so do those of t
latter that a similar state of things prevails in respect of t
readiness and general trustworthiness of the memory. T
evidence is not so direct and conclusive in the latter ca
for the memory is not singled out and subjected to measur
ment by itself, but is taken in combination with innumeral
other influencing circumstances. Still there can be lit
doubt that the statistics tell on the whole in this directi
and that by duly varying and extending them they m
obtain considerable probative force.

The fact is that Probability has nothing more to do wi
Natural Theology, either in its favour or against it, than

eral principles of Logic or Induction have. It is simply a y of rules for drawing inferences about classes of events ch are distinguished by a certain quality. The believer ı Deity will, by the study of nature, be led to form an ıion about His works, and so to a certain extent about attributes. But it is surely unreasonable to propose t he should abandon his belief because the sequence of nts,—not, observe, their general tendency towards happis or misery, good or evil,—is brought about in a way erent from what he had expected; whether it be by disying order where he had expected irregularity, or by olving the machinery of secondary causes where he had ected immediate agency.

§ 8. It is both amusing and instructive to consider ıt very different feelings might have been excited in our ıds by this co-existence of, what may be called, ignorance individuals and knowledge of aggregates, if they had preted themselves to our observation in a reverse order. ng utterly unable to make assured predictions about a gle life, or the conduct of individuals, people are someıes startled, and occasionally even dismayed, at the unected discovery that such predictions can be confidently de when we are speaking of large numbers. And so ıe are prompted to exclaim, This is denying Providence! s utter Fatalism! But let us assume, for a moment, that familiarity with the subject had been experienced, in the t instance, in reference to the aggregates instead of the ividual lives. It is difficult, perhaps, to carry out such a position completely; though we may readily conceive ıething approaching to it in the case of an ignorant clerk ı Life Assurance Office, who had never thought of life, ext as having such a 'value' at such an age, and who had dly estimated it except in the form of averages. Might we not suppose him, in some moment of reflectiveness, bei astonished and dismayed at the sudden realization of th utter uncertainty in which the single life is involved? A might not his exclamation in turn be, Why this is denyi Providence! It is utter chaos and chance! A belief in Creator and Administrator of the world is not confined any particular assumption about the nature of the immedi sequence of events, but those who have been accustom hitherto to regard the events under one of the aspects abo referred to, will often for a time feel at a loss how to conne them with the other.

§ 9. So far we have been touching on a very gene question; viz. the relation of the fundamental postulates Probability to the conception of Order or Uniformity in th world, physical or moral. The difficulties which thence ari are mainly theological, metaphysical or psychological. Wh we must now consider are problems of a more detailed logical character. They are prominently these two; (1) th distinction between chance arrangement and *causal* arrang ment in physical phenomena; and (2) the distinction b tween chance arrangement and *designed* arrangement whe we are supposed to be contemplating rational agency acting on one side at least.

II. The first of these questions raises the antithe between chance and causation, not as a general characteristi pervading all phenomena, but in reference to some specifi occurrence:—Is this a case of chance or not? The mo strenuous supporters of the universal prevalence of causatio and order admit that the question is a relevant one, an they must therefore be supposed to have some rule fo testing the answers to it.

Suppose, for instance, a man is seized with a fit in house where he has gone to dine, and dies there; and som e remarks that that was the very house in which he was rn. We begin to wonder if this was an odd coincidence d nothing more. But if our informant goes on to tell us t the house was an old family one, and was occupied by brother of the deceased, we should feel at once that se facts put the matter in a rather different light. Or in, as Cournot suggests, if we hear that two brothers ve been killed in battle on the same day, it makes a great ference in our estimation of the case whether they were led fighting in the same engagement or whether one fell the north of France and the other in the south. The ter we should at once class with mere coincidences, whereas former might admit of explanation.

§ 10. The problem, as thus conceived, seems to be one her of Inductive Logic than of Probability, because there not the slightest attempt to calculate chances. But it serves some notice here. Of course no accurate thinker o was under the sway of modern physical notions would a moment doubt that each of the two elements in question d its own 'cause' behind it, from which (assuming perfect owledge) it might have been confidently inferred. No re would he doubt, I apprehend, that if we could take a ficiently minute and comprehensive view, and penetrate ficiently far back into the past, we should reach a stage at ich (again assuming perfect knowledge) the coexistence of two events could equally have been foreseen. The ployment of the word *casual* therefore does not imply any ection of a cause; but it does nevertheless correspond to a tinction of some practical importance. We call a coinci- ce casual, I apprehend, when we mean to imply that no owledge of one of the two elements, which we can suppose be practically attainable, would enable us to expect the er. We know of no generalization which covers them both, except of course such as are taken for granted to b inoperative. In such an application it seems that the wor 'casual' is not used in antithesis to 'causal' or to 'designed', but rather to that broader conception of order or regularit to which I should apply the term Uniformity. The casu coincidence is one which we cannot bring under any speci generalization; certain, probable, or even plausible.

A slightly different way of expressing this distinction i to regard these 'mere coincidences' as being simply cases i point of *independent* events, in the sense in which independence was described in a former chapter. We sa that any two events, A and B, were so described when eac happens with precisely the same relative statistical frequen whether the other happens or not. This state of thin seems to hold good of the successions of heads and tails i tossing coins, as in that of male and female births in town, or that of the digits in many mathematical tabl Thus we suppose that when men are picked up in the stre and taken into a house to die, there will not be in the lon run any preferential selection for or against the house i which they were born. And all that we necessarily mean t claim when we deny of such an occurrence, in any particul case, that it is a mere coincidence, is that that particul case must be taken out of the common list and transferre to one in which there *is* some such preferential selection.

§ 11. III. The next problem is a somewhat more intricate one, and will therefore require rather careful subdivision. It involves the antithesis between Chance an Design. That is, we are not now (as in the preceding case) considering objects in their physical aspect alone, and takin account only of the relative frequency of their coexistence o sequence; but we are considering the agency by which th are produced, and we are enquiring whether that agency to what we call chance, or whether it employed what design.

reader must clearly understand that we are not now ng the mere question of fact whether a certain d arrangement *is* what we call a chance one. This, fully pointed out in the fourth chapter, can be settled e inspection, provided the materials are extensive . What we are now proposing to do is to carry on uiry from the point at which we then had to leave it solving the question, Given a certain arrangement, is likely that this was *produced* by design, or by some methods commonly called chance methods? The dis- will be obvious if we revert to the succession of which constitute the ratio π. As I have said, this ment, regarded as a mere succession of digits, appears l perfectly the characteristics of a chance arrange- If we were to omit the first four or five digits, are familiar to most of us, we might safely defy any whom it was shown to say that it was not got at by drawing figures from a bag. He might look at it for le life without detecting that it was anything but the of such a chance selection. And rightly so, because d as a mere arrangement it *is* a chance one: it fulfils requirements of such an arrangement[1]. The question

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[1] bts have been expressed e truly random character ligits in this case (v. De *Budget of Paradoxes*, p. Jevons has gone so far as *rinciples of Science*, p. 529), hould the value of π, when l to a great number of ontain the digit 7 much less y than any other digit!" quite understand what this means. If such a question were asked in relation to any unusual divergence from the *à priori* chance in a case of throwing dice, say, we should probably substitute for it the following, as being more appropriate to our science:—Assign the degree of improbability of the event in question; i.e. its statistical rarity. And we should then proceed to judge, in the way indicated in the

we are now proceeding to discuss is this: Given a
arrangement how are we to determine the process b
it was arrived at?

We are supposed to have some event before u
might have been produced in either of two alt
ways, i.e. by chance or by some kind of deliberate
and we are asked to determine the odds in favour
or other of these alternatives. It is therefore a pr
Inverse Probability and is liable to all the diffic
which problems of this class are apt to be exposed.

text, whether this improbability gave rise to any grounds of suspicion.

The calculation is simple. The actual number of 7's, in the 708 digits, is 53: whilst the fair average would be 71. The question is, What is the chance of such a departure from the average in 708 turns? By the usual methods of calculation (v. Galloway on *Probability*) the chances against an excess or defect of 18 are about 44 : 1, in respect of any specified digit. But of course what we want to decide are the chances against *some one of the ten* showing this divergence. This I estimate as being approximately determined by the fraction $(\frac{44}{45})^{10}$, viz. ·8. This represents odds of only about 4 : 1 against such an occurrence, which is nothing remarkable. As a matter of fact several digits in the two other magnitudes which Mr Shanks had calculated to the same length, viz. $\mathrm{Tan}^{-1}\frac{1}{5}$ and $\mathrm{Tan}^{-1}\frac{1}{239}$, show the same divergencies (v. *Proc. Roy. Soc.* xxi. 319).

I may call attention here to a point which should have been noticed
in the chapter on Random
must be cautious when
upon the random characte
inspection. It is very i
here to compare the digits
those within the 'period' c
lating decimal of very lo
That of 1÷7699, which
full period of 7698 figures
culated some years ago by
bridge graduates (Mr Lun
Suffield), and privately prin
confine our examination to
of the succession the ran
racter seems plausible; i.e.
and their various combinat
out in nearly, but not exa
numbers. So if we take
10; the averages hover ni
45. But if we took the wh
which 'circulates,' we sh
these characteristics over
the random character w
appear. That is, instead o
ultimate approximation t
we should have (as far a
possible) an absolute a
of it.

§ 12. For the theoretic solution of such a question we require the two following data:—

(1) The relative frequency of the two classes of agencies, viz. that which is to act in a chance way and that which is to act designedly.

(2) The probability that each of these agencies, if it were the really operative one, would produce the event in question.

The latter of these data can generally be secured without any difficulty. The determination of the various contingencies on the chance hypothesis ought not, if the example were a suitable one, to offer any other than arithmetical difficulties. And as regards the design alternative, it is generally taken for granted that if this had been operative it would certainly have produced the result aimed at. For instance, if ten pence are found on a table, all with head uppermost, and it be asked whether chance or design had been at work here; we feel no difficulty up to a certain point. Had the pence been tossed we should have got ten heads only once in 1024 throws; but had they been placed designedly the result would have been achieved with certainty.

But the other postulate, viz. that of the relative prevalence of these two classes of agencies, opens up a far more serious class of difficulties. Cases can be found no doubt, though they are not very frequent, in which this question can be answered approximately, and then there is no further trouble. For instance, if in a school class-list I were to see the four names Brown, Jones, Robinson, Smith, standing in this order, it might occur to me to enquire whether this arrangement were alphabetical or one of merit. In our enlarged sense of the terms this is equivalent to chance and design as the alternatives; for, since the initial letter of a boy's name has no known connection with his attainment the successive arrangement of these letters on any othe than the alphabetical plan will display the random feature just as we found to be the case with the digits of an incom mensurable magnitude. The odds are 23 to 1 against 4 names coming undesignedly in alphabetical order; they ar equivalent to certainty in favour of their doing so if thi order had been designed. As regards the relative frequenc of the two kinds of orders in school examinations I do no know that statistics are at hand, though they could easil be procured if necessary, but it is pretty certain that th majority adopt the order of merit. Put for hypothesis th proportion as high as 9 to 1, and it would still be found mo likely than not that in the case in question the order wa really an alphabetical one.

§ 13. But in the vast majority of cases we have n such statistics at hand, and then we find ourselves expose to very serious ambiguities. These may be divided int two distinct classes, the nature of which will best be seen by the discussion of examples.

In the first place we are especially liable to the draw back already described in a former chapter as renderin mere statistics so untrustworthy, which consists in the fac that the proportions are so apt to be disturbed almost fro moment to moment by the possession of fresh hints or infor mation. We saw for instance why it was that statistics o mortality were so very unserviceable in the midst of disease or in the crisis of a battle. Suppose now that o coming into a room I see on the table ten coins lying fac uppermost, and am asked what was the likelihood that th arrangement was brought about by design. Everythin turns upon special knowledge of the circumstances of th case. Who had been in the room? Were they childre oin-collectors, or persons who might have been supposed ave indulged in tossing for sport or for gambling pur- s? Were the coins new or old ones? a distinction of kind would be very pertinent when we were considering existence of any motive for arranging them the same uppermost. And so on; we feel that our statistics are e mercy of any momentary fragment of information.

14. But there is another consideration besides this. only should we be thus influenced by what may be l external circumstances of a general kind, such as the cter and position of the agents, we should also be in- ed by what we supposed to be the conventional[1] estimate which this or that particular chance arrangement was regarded. Thus from time to time as new games of become popular new combinations acquire significance; herefore when the question of design takes the form of le cheating a knowledge of the current estimate of combinations becomes exceedingly important.

15. The full significance of these difficulties will best pprehended by the discussion of a case which is not ious or invented for the purpose, but which has actually rise to serious dispute. Some years ago Prof. Piazzi h published a work[2] upon the great pyramid of Ghizeh, eneral object of which was to show that that building ined, in its magnitude, proportions and contents, a er of almost imperishable natural standards of length, e, &c. Amongst other things it was determined that

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course this conventional esti- is nothing different in kind hat which may attach to *any* r succession. Ten heads in ion is intrinsically or ob- y indistinguishable in cha- rom alternate heads and tails, or seven heads and three tails, &c. Its distinction only consists in its almost universal acceptance as remarkable.

[2] *Our Inheritance in the Great Pyramid*, Ed. III. 1877.

the value of π was accurately (the degree of accuracy is n I think, assigned) indicated by the ratio of the sides to t height. The contention was that this result could not accidental but must have been designed.

As regards the estimation of the value of the chan hypothesis the calculation is not quite so clear as in t case of dice or cards. We cannot indeed suppose th for a given length of base, *any* height can be equally possib We must limit ourselves to a certain range here; for if t high the building would be insecure, and if too low it wou be ridiculous. Again, we must decide to how close approximation the measurements are made. If they guaranteed to the hundredth of an inch the coincide would be of a quite different order from one where t guarantee extended only to an inch. Suppose that th has been decided, and that we have ascertained that o of 10,000 possible heights for a pyramid of given base j that one has been selected which would most nearly yie the ratio of the radius to the circumference of a circle.

The remaining consideration would be the relative fr quency of the 'design' alternative,—what is called its *priori* probability,—that is, the relative frequency wi which such builders can be supposed to have aimed at th ratio; with the obvious implied assumption that if they d aim at it they would certainly secure it. Considering o extreme ignorance of the attainments of the builders it obvious that no attempt at numerical appreciation is he possible. If indeed the 'design' was interpreted to me conscious resolve to produce that ratio, instead of mere solve to employ some method which happened to prod it, few persons would feel much hesitation. Not only we feel tolerably certain that the builders did not know value of π, except in the rude way in which all artific ust know it; but we can see no rational motive, if they d know it, which should induce them to perpetuate it their building. If, however, to adopt an ingenious sug-stion[1], we suppose that the builder may have proceeded the following fashion, the matter assumes a different pect. Suppose that having decided on the height of his ramid he drew a circle with that as radius: that, laying wn a cord along the line of this circle, he drew this cord it into a square, which square marked the base of the ilding. Hardly any simpler means could be devised in comparatively rude age; and it is obvious that the cir-umference of the base, being equal to the length of the rd, would bear exactly the admitted ratio to the height. n other words, the exact attainment of a geometric value oes not imply a knowledge of that ratio, but merely of some ethod which involves and displays it. A teredo can bore, s well as any of us, a hole which displays the geometric pro-erties of a circle, but we do not credit it with correspond-ng knowledge.

As before said, all numerical appreciation of the like-ihood of the design alternative is out of the question. But, f the precision is equal to what Mr Smyth claimed, I sup-ose that most persons (with the above suggestion before hem) will think it somewhat more likely that the coinci-ence was not a chance one.

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[1] Made in *Nature* (Jan. 24, 1878) y Mr J. G. Jackson. It must be marked that Mr Smyth's alter-ative statement of his case leads up that explanation:—"The vertical ight of the great pyramid is the dius of a theoretical circle the ngth of whose curved circumference exactly equal to the sum of the lengths of the four straight sides of the actual and practical square base." As regards the alternatives of chance and design, here, it must be remembered in justice to Mr Smyth's argument that the antithesis he admits to chance is not human, but divine design.

§ 16. There still remains a serious, and highly intere speculative consideration. In the above argument we it for granted, in calculating the chance alternative, only *one* of the 10,000 possible values was favourable; is, we took it for granted that the ratio π was the onl whose claims, so to say, were before the court. But clear that if we had obtained just double this ratio the would have been of similar significance, for it would been simply the ratio of the circumference to the dian In fact, Mr Smyth's selected ratio,—the height to twic breadth of the base as compared with the diameter t circumference,—is obviously only one of a plurality of r Again; if the measured results had shown that the ra the height to one side of the base was $1 : \sqrt{2}$ (i.e. that side to a diagonal of a square) or $1 : \sqrt{3}$ (i.e. that of a to a diagonal of a cube) would not such results equally evidence of design? Proceeding in this way, we n suggest one known mathematical ratio after another most of the 10,000 supposed possible values had been t into account. We might then argue thus: since al every possible height of the pyramid would correspo *some* mathematical ratio, a builder, ignorant of the alike, would be not at all unlikely to stumble upon o other of them: why then attribute design to him in one rather than another?

§ 17. The answer to this objection has been al hinted at. Everything turns upon the *conventional* esti of one result as compared with another. Revert, for plicity, to the coins. Ten heads is just as likely as alte heads and tails, or five heads followed by five tails; fact, as any one of the remaining 1023 possible cases. universal convention has picked out a run of ten as remarkable. Here, of course, the convention seems a

and indeed inevitable one, but in other cases it is
arbitrary. For instance, in cards, "queen. of spades
ave of diamonds" is exactly as uncommon as any
such pair: moreover, till *bezique* was introduced it
presumably no superior interest over any other
d pair. But during the time when that game was
pular this combination was brought into the category
cidences in which interest was felt; and, given dis-
y amongst the players, its chance of being designed
t once on a much better footing[1].

turning then to the pyramid, we see that in balancing
ims of chance and design we must, in fairness to the
reckon to its account several other values as well as
π, e.g. $\sqrt{2}$ and $\sqrt{3}$, and a few more such simple and
r ratios, as well as some of their multiples. But
the number of such values which *might* be reckoned,
ground that they are actually known to us, is infinite,
e number that *ought* to be reckoned, on the ground
hey could have been familiar to the builders of a
d, are very few. The order of probability for or against
stence of design will not therefore be seriously altered
y such considerations[2].

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ournot, *Essai sur les fonde-*
nos connaissances. Vol. I.

deserves notice that con-
ns of this kind have found
ay into the Law Courts
of course without any at-
numerical valuation. Thus,
elebrated De Ros trial, in
the evidence was indirect,
ground of suspicion seems
been that Lord De Ros,
aling at whist, obtained far
more court cards than chance could be expected to assign him; and that in consequence his average gains for several years in succession were unusually large. The counsel for the defence urged that still larger gains had been secured by other players without suspicion of unfairness,—(I cannot find that it was explained over how large an area of experience these instances had been sought; nor how far the magnitude of the stakes, as distinguished from the

§ 18. Up to this point it will be observed that wh have been balancing against each other are two for agency,—of human agency, that is,—one acting th chance, and the other by direct design. In this ca know where we are, for we can thoroughly understand a of this kind. The problem is indeed but seldom numer soluble, and in most cases not soluble at all, but it is a rate capable of being clearly stated. We know the k answer to be expected and the reasons which would se determine it, if they were attainable.

The next stage in the enquiry would be that of bala ordinary human chance agency against,—I will not c direct spiritualist agency, for that would be narrowin hypothesis unnecessarily,—but against all other pc causes. Some of the investigations of the Societ Psychical Research will furnish an admirable illustrat what is intended by this statement. There is a ful cussion of these applications in a recent essay by Mr Edgeworth[1]; but as his account of the matter is con with other calculations and diagrams I can only quote part. But I am in substantial agreement with him.

"It is recorded that 1833 guesses were made by a cipient' as to the suit of cards which the 'agent' had upon. The number of successful guesses was 510, con ably above 458, the number which, as being the quar 1833, would, on the supposition of pure chance, be likely than any other number. Now, by the Law of we are able approximately to determine the probabil

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number of successes, accounted for that of the actual gains),—and that large allowance must be made for skill where the actual gains were computed. (See the *Times*' report, Feb. 11, 1837.)

[1] *Metretike.* At the end volume will be found a use of a number of other publi by the same author on allied

ı an excess occurring by chance. It is equal to the exnity of the tail of a probability-curve such as [those we e already had occasion to examine].... The proportion this extremity of the tail to the whole body is ·003 to 1. ıt fraction, then, is the probability of a chance shot king that extremity of the tail; the probability that, if guessing were governed by pure chance, a number of cessful guesses equal or greater than 510 would occur": ls, that is, of about 332 to 1 against such occurrence.

§ 19. Mr Edgeworth holds, as strongly as I do, that for purıes of calculation, in any strict sense of the word, we ought ıave some determination of the data on the non-chance side the hypothesis. We ought to know its relative frequency occurrence, and the relative frequency with which it ains its aims. I am also in agreement with him that hat that other cause may be,—whether some trick, or onscious illusion, or *thought-transference* of the sort which indicated by the investigators—it is for common-sense l ordinary Logic to consider."

I am in agreement therefore with those who think that ugh we cannot form a quantitative opinion we can in tain cases form a tolerably decisive one. Of course if we ›w the last word to the supporters of the chance hyposis we can never reach proof, for it will always be open to m to revise and re-fix the antecedent probability of the nter hypothesis. What we may fairly require is that se who deny the chance explanation should assign some ; of minimum value to the probability of occurrence on other supposition, and we can then try to surmount this increasing the rarity of the actually produced phenomenon the chance hypothesis. If, for instance, they declare that heir estimation the odds against any other than the chance ncy being at work are greater than 332 to 1, we must try to secure a yet uncommoner occurrence than th question. If the supporters of thought-transference the courage of their convictions,—as they most assu have,—they would not shrink from accepting this tes am inclined to think that even at present, on such evic as that above, the probability that the results were got ordinary guessing is very small.

§ 20. The problems discussed in the preceding sec are at least intelligible even if they are not always vable. But before finishing this chapter we must take n of some speculations upon this part of the subject which d seem to keep quite within the limits of what is intelli Take for instance the question discussed by Arbuthno a paper in the *Phil. Transactions*, Vol. XXVII.) under title "An Argument for Divine Providence, taken fro constant Regularity observed in the birth of both sexes." his argument been of the ordinary teleological kind; is, had he simply maintained that the existent ratio o proximate equality, with a six per cent. surplusage of m was a beneficent one, there would have been nothing he object against. But what he contemplated was just su balance of alternate hypotheses between chance and d as we are here considering. His conclusion in his own is, "it is art, not chance, that governs."

It is difficult to render such an argument precise wi rendering it simply ridiculous. Strictly understood i surely bear only one of two interpretations. On the hand we may be personifying Chance: regarding it a agent which must be reckoned with as being quite ca of having produced man, or at any rate having arrange proportion of the sexes. And then the decision mu drawn, as between this agent and the Creator, which o two produced the existent arrangement. If so, and Ch ıed as any agent which produces a chance or random ment, I am afraid there can be little doubt that it ıs agent that was at work in the case in question. ·angement of male and female births presents, so far an see, one of the most perfect examples of chance: s ultimate uniformity emerging out of individual ırity: all the 'runs' or successions of each alternative .y represented: the fact of, say, five sons having been born in a family does not seem to have any certain ı diminishing the likelihood of the next being a son, on. Such a nearly perfect instance of 'independent is comparatively very rare in physical phenomena. ! that we can claim from a chance arrangement[1]. The her interpretation I can see is to suggest that there t one agent who might, like any one of us, have either ıp or designed, and we have to ascertain which course ɔably adopted in the case in question. Here too, if to judge of his mode of action by the tests we should o any work of our own, it would certainly look very .s if he had adopted some scheme of tossing.

1. The simple fact is that any rational attempt to

; is, if we look simply to l results, as Arbuthnott did, ·e should do if we were ex- the tosses of a penny. If ırkable theory of Dr Düsing *gulierung des Geschlechts- sses...* Jena, 1834) be con- the matter would assume hat different aspect. He at- to show, both on physio- rounds, and by analysis of referring to men and that there is a decidedly *:tory* process at work. That any cause either sex attains a preponderance, agencies are at once set in motion which tend to redress the balance. This is a modification and improvement of the older theory, that the relative age of the parents has something to do with the sex of the offspring.

Quetelet (*Letters*, p. 61) has attempted to *prove* a proposition about the succession of male and female births by certain experiments supposed to be tried upon an urn with black and white balls in it. But this is going too far. (See the note at the end of this chapter.)

decide between chance and design as agencies must be co fined to the case of finite intelligences. One of the im portant determining elements here, as we have seen, is t state of knowledge of the agent, and the conventional es mate entertained about this or that particular arrangem and these can be appreciated only when we are dealing wi beings like ourselves.

For instance, to return to that much debated questi about the arrangement of the stars, there can hardly be doubt that what Mitchell,—who started the discussion,— in view was the decision between Chance and Design. says (*Trans. Roy. Soc.* 1767) "The argument I intend make use of...is of that kind which infers either design some general law from a general analogy and from the gre ness of the odds against things having been in the pres situation if it was not owing to some such cause." And concludes that had the stars "been scattered by mere cha as it might happen" there would be "odds of near 500, to 1 that no six stars out of that number [1500], scattered random in the whole heavens, would be within so small distance from each other as the Pleiades are." Under such interpretation the controversy seems to me to be id I do not for a moment dispute that there is some force the ordinary teleological argument which seeks to trace sig of goodness and wisdom in the general tendency of thin But what do we possibly understand about the nature creation, or the designs of the Creator, which should ena us to decide about the likelihood of his putting the stars one shape rather than in another, or which should allow significance to "mere chance" as contrasted with his posed all-pervading agency?

§ 22. Reduced to intelligible terms the two followi questions seem to me to emerge from the controversy:—

) The stars being distributed through space, some of would of course be nearly in a straight line behind s when looked at from our planet. Supposing that were tolerably uniformly distributed, we could calculate how many of them would thus be seen in apparent proximity to one another. The question is then put, here more of them near to each other, two and two, such calculation would account for? The answer is that are many more. So far as I can see the only direct nce that can be drawn from this is that they are *not* rmly distributed, but have a tendency to go in pairs. however, is a perfectly sound and reasonable applica- of the theory. Any further conclusions, such as that pairs of stars will form systems, as it were, to them- , revolving about one another, and for all practical pur- unaffected by the rest of the sidereal system, are of derived from astronomical considerations[1]. Probability es itself to the simple answer that the distribution is niform; it cannot pretend to say whether, and by what al process, these binary systems of stars have been d'[2].

23 (II). The second question is this, Does the distri- of the stars, after allowing for the case of the binary

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is precisely analogous to the sion that the *flowers* of the (as distinguished from the v. p. 109) are not distributed lom, but have a tendency to groups of two or more. Mere tion shows this: and then, ur knowledge of the growth of we may infer that these little spring from the same root.

this discussion, writers often speak of the probability of a "*physical connection*" between these double stars. The phrase seems misleading, for on the usual hypothesis of universal gravitation *all* stars are physically connected, by gravitation. It is therefore better, as above, to make it simply a question of relative proximity, and to leave it to astronomy to infer what follows from unusual proximity.

stars just mentioned, resemble that which would b duced by human agency sprinkling things 'at ran (We are speaking, of course, of their distribution as pears to us, on the visible heavens, for this is nearly a we can observe; but if they extend beyond the tel range in every direction, this would lead to practically the same discussion as if we considered their actual ar ment in space.) We have fully discussed, in a former ter, the meaning of 'randomness.' Applying it to th before us, the question becomes this, Is the distri tolerably uniform on the whole, but with innumerabl vidual deflections? That is, when we compare large are the ratios of the number of stars in each equa approximately equal, whilst, as we compare small smaller areas, do the relative numbers become mo more irregular? With certain exceptions, such as t the Milky Way and other nebular clusters, this seems pretty much the case, at any rate as regards the bulk stars[1].

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[1] Professor Forbes in the paper in the Philosophical Magazine already referred to (Ch. VII. § 18) gave several diagrams to show what were the actual arrangements of a random distribution. He scattered peas over a chess-board, and then counted the number which rested on each square. His figures seem to show that the general appearance of the stars is much the same as that produced by such a plan of scattering.

Some recent investigations by Mr R. A. Proctor seem to show, however, that there are at least two exceptions to this tolerably uniform distribution. (1) He has ascertained that the stars are decided thickly aggregated in the Mil than elsewhere. So far as t be relied on the argument same as in the case of the stars; it tends to prove t proximity of the stars in th Way is not merely appar actual. (2) He has ascertai there are two large areas, North and South hemisph which the stars are muc thickly aggregated than el Here, it seems to me, Pro proves nothing: we are simp ing that the distribution is What may follow in the wa

All further questions: the decision, for instance, for or gainst any form of the Nebular Hypothesis: or, admitting is, the decision whether such and such parts of the visible eavens have sprung from the same nebula, must be left to stronomy to adjudicate.

NOTE ON THE PROPORTIONS OF THE SEXES.

The following remarks were rather too long for convenient insertion on 259, and are therefore appended here.

The 'random' character of male and female births has generally been sted almost entirely on statistics of place and time. But what is more nted, surely, is the proportion displayed when we compare a number of *ilies.* This seems so obvious that I cannot but suppose that the investiion must have been already made somewhere, though I have not found y trace of it in the most likely quarters. Thus Prof. Lexis (*Massenereinungen*) when supporting his view that the proportion between the xes at birth is almost the only instance known to him, in natural phenona, of true normal dispersion about a mean, rests his conclusions on the dinary statistics of the registers of different countries.

It certainly needs proof that the same characteristics will hold good hen the family is taken as the unit, especially as some theories (e.g. that Sadler) would imply that 'runs' of boys or girls would be proportionally mmoner than pure chance would assign. Lexis has shown that this is ost markedly the case with *twins*: i.e., to use an obviously intelligible tation, (M for male, F for female), that M.M. and F.F. are very much mmoner in proportion than M.F.

I have collected statistics including over 13,000 male and female births, ranged in families of four and upwards. They were taken from the digrees in the Herald's Visitations, and therefore represent as a rule a mewhat select class, viz. the families of the eldest sons of English country ntlemen in the sixteenth century. They are not sufficiently extensive yet r publication, but I give a summary of the results to indicate their tendency far. The upper line of figures in each case gives the *observed* results: i.e.

ences as to the physical process of ation by which the stars have n disposed is a question for the stronomer. See Mr Proctor's *Essays on Astronomy*, p. 297. Also a series of Essays in *The Universe and the coming Transits.*

in the case of a family of four, the numbers which had four male, thre and one female, two male and two female, and so on. The lower lin the *calculated* results; i.e. the corresponding numbers which woul been obtained had batches of M.s and F.s been drawn from a bag in they were mixed in the ratio assigned by the total observed numb those families.

512 families of 4;	m^4 m^3f m^2f^2 mf^3 f^4
yielding	81 + 148 + 161 + 98 + 24 (observed.)
1188 M.: 860 F.	57 + 168 + 184 + 88 + 15 (calculated.)
512 families of 5;	m^5 m^4f m^3f^2 m^2f^3 mf^4 f^5
yielding	50 + 82 + 161 + 143 + 61 + 15 (obs.)
1402 M.: 1158 F.	25 + 103 + 172 + 143 + 59 + 10 (calc.)
512 families of 6;	m^6 m^5f m^4f^2 m^3f^3 m^2f^4 mf^5 f^6
yielding	30 + 48 + 115 + 146 + 126 + 40 + 7 (
1612 M.: 1460 F.	10 + 56 + 133 + 159 + 108 + 41 + 5 (

The numbers for the larger families are as yet too small to be giving, but they show the same tendency. It will be seen that in eve the observed central values are less than the calculated; and th observed extreme values are much greater than the calculated. The seem to suggest (so far) that a family cannot be likened to a chance d of the requisite number from *one* bag. A better analogy would be to s two bags, one with M.s in excess and the other with F.s in less exce that some persons draw from one and some from the other. But statistics are needed.

It will be observed that the total excess of male births is large. *may* arise from undue omission of females; but I have carefully c myself to the two or three last generations, in each pedigree, for security.

CHAPTER XI.

CERTAIN CONSEQUENCES OF THE OBJECTIVE TREATMENT OF A SCIENCE OF INFERENCE[1].

1. STUDENTS of Logic are familiar with that broad
nction between the two methods of treatment to which
names of Material and Conceptualist may be applied.
distinction was one which had been gradually growing
nder other names before it was emphasized, and treated
distinction within the field of Logic proper, by the pub-
on of Mill's well known work. No one, for instance,
read Whewell's treatises on Induction, or Herschel's
ourse, without seeing that they are treating of much the
subject matter, and regarding it in much the same
as that which Mill discussed under the name of Logic,
gh they were not disposed to give it that name. That
ese writers throughout took it for granted that what
had to do was to systematise the facts of nature in
objective form, and under their widest possible treat-
, and to expound the principal modes of inference and
principal practical aids in the investigation of these

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[1] the previous edition a large
f this chapter was devoted to
neral consideration of the dis-
n between a Material and a
ptualist view of Logic. I have
d most of this here, as also a
part of a chapter devoted to the
detailed discussion of the Law of Causation, as I hope before very long to express my opinions on these subjects more fully, and more appropriately, in a treatise on the general principles of Inductive Logic.

modes of inference, which reason could suggest and whic experience could justify. What Mill did was to bring the methods into close relation with such portions of the o scholastic Logic as he felt able to retain, to work them o into much fuller detail, to systematize them by giving the a certain philosophical and psychological foundation,—an to entitle the result *Logic.*

The practical treatment of a science will seldom correspond closely to the ideal which its supporters propose t themselves, and still seldomer to that which its antagoni insist upon demanding from the supporters. If we were t take our account of the distinction between the two views o Logic expounded respectively by Hamilton and by Mi from Mill and Hamilton respectively, we should certainl not find it easy to bring them under one common definitio By such a test, the material Logic would be regarded a nothing more than a somewhat arbitrary selection from th domain of Physical Science in general, and the conceptuali Logic nothing more than a somewhat arbitrary selection fro the domain of Psychology. The former would omit all consideration of the laws of thought and the latter all consideration of the truth or falsehood of our conclusions.

Of course, in practice, such extremes as these are soo seen to be avoidable, and in spite of all controversial exaggerations the expounders of the opposite views do contrive t retain a large area of speculation in common. I do not propose here to examine in detail the restrictions by which thi accommodation is brought about, or the very real and important distinctions of method, aim, tests, and limits whic in spite of all approach to agreement are still found to subsist. To attempt this would be to open up rather too wide a enquiry to be suitable in a treatise on one subdivision only of the general science of Inference.

§ 2. One subdivision of this enquiry is however really forced upon our notice. It does become important to consider the restrictions to which the ultra-material account of the province of Logic has to be subjected, because we shall thus have our attention drawn to an aspect of the subject which, slight and fleeting as it is within the region of Induction becomes very prominent and comparatively permanent in that of Probability. According to this ultra-material view, Inductive Logic would generally be considered to have nothing to do with anything but objective facts: its duty is to start from facts and to confine itself to such methods as will yield nothing but facts. What is doubtful it either establishes or it lets alone for the present, what is unattainable it rejects, and in this way it proceeds to build up by slow accretion a vast fabric of certain knowledge.

But of course all this is supposed to be done by human minds, and therefore if we enquire whether notions or concepts.—call them what we will,—have no place in such a scheme it must necessarily be admitted that they *have* some place. The facts which form our starting point must be grasped by an intelligent being before inference can be built upon them; and the 'facts' which form the conclusion have often, at any rate for some time, no place anywhere else than in the mind of man. But no one can read Mill's treatise, for instance, without noticing how slight is his reference to this aspect of the question. He remarks, in almost contemptuous indifference, that the man who digs must of course have a notion of the ground he digs and of the spade he puts into it, but he evidently considers that these 'notions' need not much more occupy the attention of the speculative logician, in so far as his mere inferences are concerned, than they occupy that of the husbandman.

§ 3. It must be admitted that there is some warrant for this omission of all reference to the subjective sid inference so long as we are dealing with Inductive Lo The inductive *discoverer* is of course in a very diffe position. If he is worthy of the name his mind at ev moment will be teeming with notions which he would b far as any one from calling facts: he is busy making th such to the best of his power. But the logician who foll in his steps, and whose business it is to explain and jus what his leader has discovered, is rather apt to overlook mental or uncertain stage. What he mostly deals in are 'complete inductions' and 'well-grounded generalizati and so forth, or the exploded errors which contradict th the prisoners and the corpses respectively, which the discoverer leaves on the field behind him whilst he pre on to complete his victory. The whole method of scienc expository as contrasted with militant,—is to emphasize distinction between fact and non-fact, and to treat of li else but these two. In other words a treatise on Induc Logic *can* be written without any occasion being foun define what is meant by a notion or concept, or even to em such terms.

§ 4. And yet, when we come to look more closely, s may be detected even within the field of Inductive Lo of an occasional breaking down of the sharp distinctio question; we may meet now and then with entities (to the widest term attainable) in reference to which it wo be hard to say that they are either facts or concepti For instance, Inductive Logic has often occasion to make of Hypotheses: to which of the above two classes are th to be referred? They do not seem in strictness to belon either; nor are they, as will presently be pointed out, by means a solitary instance of the kind.

It is true that within the province of Inductive L these hypotheses do not give much trouble on this score. However vague may be the form in which they first present themselves to the philosopher's mind, they have not much business to come before us in our capacity of logicians until they are well on their way, so to say, towards becoming facts: until they are beginning to harden into that firm tangible shape in which they will eventually appear. We generally have some such recommendations given to us as that our hypotheses shall be well-grounded and reasonable. This seems only another way of telling us that however freely the philosopher may make his guesses in the privacy of his own study, he had better not bring them out into public until they can with fair propriety be termed facts, even though the name be given with some qualification, as by terming them 'probable facts.' The reason, therefore, why we do not take much account of this intermediate state in the hypothesis, when we are dealing with the inductive processes, is that here at any rate it plays only a temporary part; its appearance in that guise is but very fugitive. If the hypothesis be a sound one, it will soon take its place as an admitted fact; if not, it will soon be rejected altogether. Its state as a hypothesis is not a normal one, and therefore we have not much occasion to scrutinize its characteristics. In so saying, it must of course be understood that we are speaking as inductive logicians; the philosopher in his workshop ought, as already remarked, to be familiar enough with the hypothesis in every stage of its existence from its origin; but the logician's duty is different, dealing as he does with proof rather than with the processes of original investigation and discovery.

We might indeed even go further, and say that in many cases the hypothesis does not present itself to the reader, that is to the recipient of the knowledge, until it has ceased

to deserve that name at all. It may be first suggested
him along with the proof which establishes it, he not hav
had occasion to think of it before. It thus comes at a sin
step out of the obscurity of the unknown into the full p
session of its rights as a fact, skipping practically the int
mediate or hypothetical stage altogether. The original
vestigator himself may have long pondered over it, and k
it present to his mind, in this its dubious stage, but fin
have given it to the world with that amount of evide
which raises it at once in the minds of others to the leve
commonly accepted facts.

Still this doubtful stage exists in every hypothe
though for logical purposes, and to most minds, it ex
in a very fugitive way only. When attention has b
directed to it, it may be also detected elsewhere in Lo
Take the case, for instance, of the reference of nar
Mill gives the examples of the sun, and a battle, as
tinguished from the ideas of them which we, or child
may entertain. Here the distinction is plain and obvi
enough. But if, on the other hand, we take the cas
things whose existence is doubtful or disputed, the diffic
above mentioned begins to show itself. The case of me
extinct things, or such as have not yet come into existe
offers indeed no trouble, since of course actually *pre*
existence is not necessary to constitute a fact. The u
distinction may even be retained also in the case of myth
existences. Centaur and Griffin have as universally re
nised a significance amongst the poets, painters, and her
as lion and leopard have. Hence we may claim, even h
that our conceptions shall be 'truthful,' 'consistent
fact,' and so on, by which we mean that they are to b
accordance with universal convention upon such subj
Necessary and universal accordance is sometimes clai

o be all that is meant by 'objective,' and since universal accordance is attainable in the case of the notoriously fictitious, our fundamental distinction between fact and conception, and our determination that our terms shall refer to what is objective rather than to what is subjective, may with some degree of strain be still conceived to be tenable even here.

§ 5. But when we come to the case of disputed phenomena the difficulty re-emerges. A supposed planet or new mineral, a doubtful fact in history, a disputed theological doctrine, are but a few examples out of many that might be offered. What some persons strenuously assert, others as strenuously deny, and whatever hope there may be of speedy agreement in the case of physical phenomena, experience shows that there is not much prospect of this in the case of those which are moral and historical, to say nothing of theological. So long as those who are in agreement confine their intercourse to themselves, their 'facts' are accepted as such, but as soon as they come to communicate with others all distinction between fact and conception is lost at once, the 'facts' of one party being mere groundless 'conceptions' to their opponents. There is therefore, I think, in these cases a real difficulty in carrying out distinctly and consistently the account which the Materialist logician offers as to the reference of names. It need hardly be pointed out that what thus applies to names or terms applies equally to propositions in which particular or general statements are made involving names.

§ 6. But when we step into Probability, and treat this from the same material or Phenomenal point of view, we can no longer neglect the question which is thus presented to us. The difficulty cannot here be rejected, as referring to what is merely temporary or occasional. The intermediate condition between conjecture and fact, so far from being temporary

or occasional only, is here normal. It is just the condi
which is specially characteristic of Probability. Henc
follows that however decidedly we may reject the (
ceptualist theory we cannot altogether reject the us
Conceptualist language. If we can prove that a given
will die next year, or attain sufficiently near to proo
leave us practically certain on the point, we may speal
his death as a (future) fact. But if we merely contemp
his death as probable? This is the sort of inference
substitute for inference, with which Probability is speci
concerned. We may, if we so please, speak of 'prob
facts,' but if we examine the meaning of the words
may find them not merely obscure, but self-contradict
Doubtless there are facts here, in the fullest sense of
term, namely the statistics upon which our opinion is
mately based, for these are known and admitted by all
have looked into the matter. The same language may
be applied to that extension of these statistics by induc
which is involved in the assertion that similar statis
will be found to prevail elsewhere, for these also may ri
fully claim universal acceptance. But these statement
was abundantly shown in the earlier chapters, stand o
very different footing from a statement concerning the
dividual event; the establishment and discussion of
former belong by rights to Induction, and only the la
to Probability.

§ 7. It is true that for want of appropriate term
express such things we are often induced, indeed compel
to apply the same name of 'facts' to such individual con
gencies. We should not, for instance, hesitate to speal
the fact of the man dying being probable, possible, unlil
or whatever it might be. But I cannot help regarding s
expressions as a strictly incorrect usage arising out c

ciency of appropriate technical terms. It is doubtless ain that one or other of the two alternatives must happen, this alternative certainty is not the subject of our conplation; what we have before us is the *single* alternative, ich is notoriously uncertain. It is this, and this only, ich is at present under notice, and whose occurrence has be estimated. We have surely no right to dignify this h the name of a fact, under any qualifications, when the osite alternative has claims, not perhaps actually equal to, at any rate not much inferior to its own. Such language, already remarked, may be quite right in Inductive logic, ere we are only concerned with conjectures of such a high ree of likelihood that their non-occurrence need not be en into practical account, and which are moreover regardas merely temporary. But in Probability the conjecture y have any degree of likelihood about it; it may be just likely as the other alternative, nay it may be much less ly. In these latter cases, for instance, if the chances are y much against the man's death, it is surely an abuse of guage to speak of the 'fact' of his dying, even though we lify it by declaring it to be highly improbable. The ject-matter essential to Probability being the uncertain, can never with propriety employ upon it language which its original and correct application is only appropriate to it is actually or approximately certain.

§ 8. It should be remembered also that this state of gs, thus characteristic of Probability, is *permanent* there. long as they remain under the treatment of that science conjectures, or whatever we like to call them, never elop into facts. I calculate, for instance, the chance that ie will give ace, or that a man will live beyond a certain . Such an approximation to knowledge as is thus acred is as much as we can ever afterwards hope to get, unless we resort to other methods of enquiry. We do n in Induction, feel ourselves on the brink of some ex mental or other proof which at any moment may rai into certainty. It is nothing but a conjecture of a ce degree of strength, and such it will ever remain, so lon Probability is left to deal with it. If anything more is to be made out of it we must appeal to direct experienc to some kind of inductive proof. As we have ~~so often~~ individual facts can ~~never be determined here, but m~~ ~~ultimate tendencies~~ and averages of many events. I indeed, by a second appeal to ~~Probability~~ improve character of my conjecture, through being able to refer a narrower and better class of statistics; but its esse nature remains throughout what it was.

It appears to me therefore that the account of the Mat ist view of logic indicated at the commencement of chapter, though substantially sound, needs some slight re sideration and restatement. It answers admirably so f ordinary Induction is concerned, but needs some rev if it is to be equally applicable to that wider vie the nature and processes of acquiring knowledge wh the science of logic is considered to involve Probab also as well as Induction.

§ 9. Briefly then it is this. We regard the scie thinker, whether he be the original investigator who covers, or the logician who analyses and describes the p that may be offered, as surrounded by a world of obje phenomena extending indefinitely both ways in time, an every direction in space. Most of them are, and always remain, unknown. If we speak of them as facts we n that they are potential objects of human knowledge, under appropriate circumstances men could come to de minate and final agreement about them. The scientifi material logician has to superintend the process of converting as much as possible of these unknown phenomena into what are known, of aggregating them, as we have said above, about the nucleus of certain data which experience and observation had to start with. In so doing his principal resources are the Methods of Induction, of which something has been said in a former chapter; another resource is found in the Theory of Probability, and another in Deduction.

Now, however such language may be objected to as savouring of Conceptualism, I can see no better compendious way of describing these processes than by saying that we are engaged in getting at conceptions of these external phenomena, and as far as possible converting these conceptions into facts. What is the natural history of 'facts' if we trace them back to their origin? They first come into being as mere guesses or conjectures, as contemplated possibilities whose correspondence with reality is either altogether disbelieved or regarded as entirely doubtful. In this stage, of course, their contrast with facts is sharp enough. *How* they arise it does not belong to Logic but to Psychology to say. Logic indeed has little or nothing to do with them whilst they are in this form. Everyone is busy all his life in entertaining such guesses upon various subjects, the superiority of the philosopher over the common man being mainly found in the quality of his guesses, and in the skill and persistence with which he sifts and examines them. In the next stage they mostly go by the name of theories or hypotheses, when they are comprehensive in their scope, or are in any way on a scale of grandeur and importance: when however they are of a trivial kind, or refer to details, we really have no distinctive or appropriate name for them, and must be content therefore to call them 'conceptions.' Through this stage they flit with great rapidity in Inductive Logic; often the

logician keeps them back until their evidence is so st
that they come before the world at once in the full dig
of facts. Hence, as already remarked, this stage of
career is not much dwelt upon in Logic. But the w
business of Probability is to discuss and estimate the
this point. Consequently, so far as this science is conce
the explanation of the Material logician as to the refe
of names and propositions has to be modified.

§ 10. The best way therefore of describing our pos
in Probability is as follows:—We are ~~*entertaining a con*~~
~~*tion*~~ of some event, past~~, present, or~~ future. From the na
~~of the case this conception~~ is all that can be actually e
tained by the mind. In its present condition it woul
incorrect to call it a fact, though we would willingly, i
could, convert it into such by making certain of it one
or the other. But so long as our conclusions are t
effected by considerations of Probability only, we canno
this. The ~~utmost we can do is~~ to *estimate* or *evalua*
The whole function of Probability is ~~to give rules~~ fo
doing. By means of ~~reference to statistics or by d~~
deduction, as the case may be, we are enabled to say
much this conception is to be believed, that is in ~~what~~
portion out of the total number of cases we shall be
in so doing. Our position, therefore, in ~~these cases s~~
distinctly that of entertaining a ~~conception, and the pr~~
~~of inference is that of ascertaining~~ to what extent we
justified in adding this conception to the already rece
body of truth and fact.

So long, then, as we are confined to Probability
conceptions remain such. But if we turn to Inductio
see that they are meant to go a step further. Their
stage is not reached until they have ~~ripened into facts~~
so taken their place amongst uncontested truths. Th

their final destination in Logic, and our task is not accomplished until they have reached it.

§ 11. Such language as this in which we speak of our position in Probability as being that of entertaining a conception, and being occupied in determining what degree of belief is to be assigned to it, may savour of Conceptualism, but is in spirit perfectly different from ~~it. Our ultimate~~ reference is always to facts. We start from ~~them as~~ our data, and reach them again eventually in our results whenever it ~~is possible.~~ In ~~Probability~~, of course, we cannot do this in the ~~individual result, but even then~~ (as shown in Ch. VI.) ~~we always~~ *justify* our conclusions by appeal to facts, viz. ~~to what happens in the long run.~~

The discussion which has been thus given to this ~~part of~~ the subject may seem somewhat tedious, but it was so obviously forced upon us when considering the distinction between the two main views of Logic, that it was impossible to pass it over without fear of misapprehension and confusion. Moreover, as will be seen in the course of the next chapter, several important conclusions could not have been properly explained and justified without first taking pains to make this part of our ground perfectly plain and satisfactory.

CHAPTER XII.

CONSEQUENCES OF THE FOREGOING DISTINCTIONS.

§ 1. We are now in a position to explain and justify some important conclusions which, if not direct consequences of the distinctions laid down in the last chapter, will at any rate be more readily appreciated and accepted after that exposition.

In the first place, it will be seen that in Probability time has nothing to do with the question; in other words, it does not matter whether the event, whose probability we are discussing, be past, present, or future. The problem before us, in its simplest form, is this:—Statistics (extended by Induction, and practically often gained by Deduction) inform us that a certain event has happened, does happen, or will happen, in a certain way in a certain proportion of cases. We form a conception of that event, and regard it as possible: but we want to do more; we want to know *how much* we ought to expect it (under the explanations given in a former chapter about quantity of belief). There is therefore a sort of relative futurity about the event, inasmuch as our knowledge of the fact, and therefore our justification or otherwise of the correctness of our surmise, almost necessarily comes after the surmise was formed; but the futurity is only relative. The evidence by which the question is to be settled may not be forthcoming yet, or we may have it by us but only consult it afterwards. It is from the fact of the futurity being, as above described, only relative, that I have preferred to speak of the conception of the event rather than of the anticipation of it. The latter term, which in some respects would have seemed more intelligible and appropriate, is open to the objection, that it does rather, in popular estimation, convey the notion of an absolute as opposed to a relative futurity.

§ 2. For example; a die is thrown. Once in six times it gives ace; if therefore we assume, without examination, that the throw is ace, we shall be right once in six times. In so doing we may, according to the usual plan, go *forwards* in time; that is, form our opinion about the throw beforehand, when no one can tell what it will be. Or we might go *backwards;* that is, form an opinion about dice that had been cast on some occasion in time past, and then correct our opinion by the testimony of some one who had been a witness of the throws. In either case the mental operation is precisely the same; an opinion formed merely on statistical grounds is afterwards corrected by specific evidence. The opinion may have been formed upon a past, present, or future event; the evidence which corrects it afterwards may be our own eyesight, or the testimony of others, or any kind of inference; by the evidence is merely meant such subsequent examination of the case as is assumed to set the matter at rest. It is quite possible, of course, that this specific evidence should never be forthcoming; the conception in that case remains as a conception, and never obtains that degree of conviction which qualifies it to be regarded as a 'fact.' This is clearly the case with all past throws of dice the results of which do not happen to have been recorded.

In discussing games of chance there are obvious advantages in confining ourselves to what is really, as well as relatively, future, for in that case direct information concerning the contemplated result being impossible, all persons are on precisely the same footing of comparative ignorance, and must form their opinion entirely from the known or inferred frequency of occurrence of the event in question. On the other hand, if the event be passed, there is almost always evidence of some kind and of some value, however slight, to inform us what the event really was; if this evidence is not actually at hand, we can generally, by waiting a little, obtain something that shall be at least of some use to us in forming our opinion. Practically ~~therefore~~ we generally confine our~~selves, in~~ anticipations of this kind, to what is really future, ~~and~~ so in popular estimation futurity ~~becomes indissolubly associated with probability.~~

§ 3. There is however an error closely connected with the above view of the subject, or at least an inaccuracy of expression which is constantly liable to lead to error, which has found wide acceptance, and has been sanctioned by writers of the greatest authority. For instance, both Butler, in his *Analogy*, and Mill, have drawn attention, under one form of expression or another, to the distinction between improbability before the event and improbability after the event, which they consider to be perfectly different things. That this phraseology indicates a distinction of importance cannot be denied, but it seems to me that the language in which it is often expressed requires to be amended.

Butler's remarks on this subject occur in his *Analogy*, in the chapter on miracles. Admitting that there is a strong presumption against miracles (his equivalent for the ordinary expression, an 'improbability before the event') he strives to obtain assent for them by showing that other events, which also have a strong presumption against them, are received on what is in reality very slight evidence. He

"There is a very strong presumption against common lative truths, and against the most ordinary facts, be- he proof of them; which yet is overcome by almost any There is a presumption of millions to one against the of Cæsar, or of any other man. For, suppose a number nmon facts so and so circumstanced, of which one had ıd of proof, should happen to come into one's thoughts, one would without any possible doubt conclude them false. And the like may be said of a single common

4. These remarks have been a good deal criticized, hey certainly seem to me misleading and obscure in reference. If one may judge by the context. and by er passage in which the same argument is afterwards ed to[1], it would certainly appear that Butler drew no ction between miraculous accounts, and other accounts , to use any of the various expressions in common use, likely or improbable or have a presumption against ; and concluded that since some of the latter were in- y accepted upon somewhat mediocre testimony, it was ther irrational to reject the former when similarly or supported[2]. This subject will come again under our , and demand fuller discussion, in the chapter on the ility of extraordinary stories. It will suffice here to

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it not self-evident that inter- robabilities of all kinds weak- rnal proof? Doubtless, but practical purpose can this be here, when it has been proved that real internal improbabi- hich rise even to moral cer- are overcome by the most testimony." Part II. ch. III. iracles must not be compared to common natural events; or to events which, though uncommon, are similar to what we daily experience; but to the extraordinary phenomena of nature. And then the comparison will be between the presumption against miracles, and the presumption against such uncommon appearances, suppose as comets,"...... Part II. ch. II.

remark that, however satisfactory such a view of the might be to some theologians, no antagonist of m would for a moment accept it. He would naturally that, instead of the miraculous element being (as considers) "a small additional presumption" again narrative, it involved the events in a totally distinct c incredibility; that it multiplied, rather than merely to, the difficulties and objections in the way of acc the account.

Mill's remarks (*Logic*, Bk. III. ch. XXV. § 4) are o ferent character. Discussing the grounds of disbel speaks of people making the mistake of "overlooki distinction between (what may be called) improb before the fact, and improbability after it, two di properties, the latter of which is always a ground belief, the former not always." He instances the th of a die. It is improbable beforehand that it shoul up ace, and yet afterwards, "there is no reason for lieving it if any credible witness asserts it." So agai chances are greatly against A. B.'s dying, yet if any o us that he died yesterday we believe it."

§ 5. That there is some difficulty about such pr as these must be admitted. The fact that so many find them a source of perplexity, and that such explanations are offered to solve the perplexity, are ficient proof of this[1]. The considerations of the last cl ever, over-technical and even scholastic as some of the uage in which it was expressed may have seemed to the er, will I hope guide us to a more satisfactory way of rding the matter.

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[1] For instance, Sir J. F. Stephen explains it by drawing a distinction between chances and probabilities, which he says that Butler has confused together; "the objection that very ordinary proof will overcome a presumption of millions to one is based upon a confusion between probabilities and chances. Th bility of an event is its cap being proved. Its chance numerical proportion betw number of possible cases— to be equally favourable—fa to its occurrence; and the of possible cases unfavoura

When we speak of an improbable event, it must be embered that, objectively considered, an event can only more or less *rare;* the extreme degree of rarity being of se that in which the event does not occur at all. Now, as shown in the last chapter, our position, when forming gments of the time in question, is that of entertaining onception or conjecture (call it what we will), and asing a certain weight of trustworthiness to it. The real inction, therefore, between the two classes of examples ectively, which are adduced both by Butler and by Mill, sists in the way in which those conceptions are obtained; being obtained in one case by the process of guessing, in the other by that of giving heed to the reports of nesses.

§ 6. Take Butler's instance first. In the 'presumption re the proof' we have represented to us a man thinking he story of Cæsar, that is, making a guess about certain orical events without any definite grounds for it, and speculating as to what value is to be attached to the ability of its truth. Such a guess is of course, as he , concluded to be false. But what does he understand the 'presumption after the proof'? That a story not pted at random, but actually suggested and supported by nesses, should be true. The latter might be accepted, lst the former would undoubtedly be rejected; but all this proves, or rather illustrates, is that the testimony

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rrence" (*General view of the Cril Law of England*, p. 255). tin, again (*Phil. Magazine*, June, 1851), employs the terms improbability and incredibility to mark the same distinction.

of almost any witness is in most cases vastly better a mere guess[1]. We may in both cases alike speak o event' if we will; in fact, as was admitted in th chapter, common language will not readily lend itself t other way of speaking. But it should be clearly ı stood that, phrase it how we will, what is really pres the man's mind, and what is to have its probable assigned to it, is the conception of an event, in the se which that expression has already been explained. surely no two conceptions can have a much more imp distinction put between them than that which is invol supposing one to rest on a mere guess, and the other report of a witness. Precisely the same remarks ap the example given by Mill. Before A. B.'s deat opinion upon the subject was nothing but a guess own founded upon life statistics; after his death founded upon the evidence of some one who presu had tolerable opportunities of knowing what the facts were.

§ 7. That the distinction before us has no essenti nection whatever with time is indeed obvious on a mo consideration. Conceive for a moment that some on opportunities of knowing whether A. B. would die o If he told us that A. B. would die to-morrow, we sho that case be just as ready to believe him as when he t that A. B. *has* died. If we continued to feel any about the statement (supposing always that we ha idence about his veracity in matters into which he had r enquired), it would be because we thought that in his , as in ours, it was equivalent to a guess, and nothing e. So with the event when past, the fact of its being t makes no difference whatever; until the credible wit- s informs us of what he knows to have occurred, we uld doubt it if it happened to come into our minds, just nuch as if it were future.

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[1] In the extreme case of the witness himself merely guessing, or being as untrustworthy as if he merely guessed, the two stories will of course stand on precisely the same footing. This case will be noticed again in Chapter XVII. It remarked that there are seve tleties here which cannot quately noticed without som ous investigation into the of the credibility of witnesse

The distinction, therefore, between probability before the nt and probability after the event seems to resolve itself ply into this;—before the event we often have no better ans of information than to appeal to statistics in some n or other, and so to guess amongst the various possible rnatives; after the event the guess may most commonly improved or superseded by appeal to specific evidence, he shape of testimony or observation. Hence, naturally, estimate in the latter case is commonly of much more ie. But if these characteristics were anyhow inverted; hat is, we were to confine ourselves to guessing about the t, and if we could find any additional evidence about the ire, the respective values of the different estimates would be inverted. The difference between these values has necessary connection with time, but depends entirely n the different grounds upon which our conception or jecture about the event in question rests.

§ 8. The following imaginary example will serve to ig out the point indicated above. Conceive a people with y short memories, and who preserved no kind of record to petuate their hold upon the events which happened ongst them[1]. The whole region of the past would then be to them what much of the future is to us; viz. a regi guesses and conjectures, one in reference to which could only judge upon general considerations of probabi rather than by direct and specific evidence. But conc also that they had amongst them a race of prophets could succeed in foretelling the future with as nea approach to accuracy and trustworthiness as our va histories, and biographies, and recollections, can attai respect to the past. The present and usual function direct evidence or testimony, and of probability, would be simply inverted; and so in consequence would the sent accidental characteristics of improbability before after the event. It would then be the latter which w by comparison be regarded as 'not always a ground o belief,' whereas in the case of the former we should have it maintained that it always was so.

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[1] According to Dante, something mbling this prevailed amongst occupants of the *Inferno*. The cardinals and others whom he there meets are able to *give* information about many events which were yet

§ 9. The origin of the mistake just discussed is v enquiring into. I take it to be as follows. It is ofte case, as above remarked, when we are speculating a a future event, and almost always the case when that f event is taken from a game of chance, that all persons a precisely the same condition of ignorance in respect t The limit of available information is confined to stati and amounts to the knowledge that the unknown e must assume some one of various alternative forms. conjecture, therefore, of any one man about it is as val as that of any other. But in regard to the past the ca very different. Here we are not in the habit of re upon statistical information. Hence the conjectures o ferent men are of extremely different values; in the ca many they amount to what we call positive knowl

to happen upon earth, but they had to *ask* it for many events which actually had happened.

is puts a broad distinction, in popular estimation, between at may be called the objective certainty of the past and of e future, a distinction, however, which from the standing-int of a science of inference ought to have no existence.

In consequence of this, when we apply to the past and e future respectively the somewhat ambiguous expression he chance of the event,' it commonly comes to bear very fferent significations. Applied to the future it bears its oper meaning, namely, the value to be assigned to a con-cture upon statistical grounds. It does so, because in this e hardly any one has more to judge by than such con-ctures. But applied to the past it shifts its meaning, ing to the fact that whereas some men have conjectures ly, others have positive knowledge. By the chance of the ent is now often meant, not the value to be assigned to a njecture founded on statistics, but to such a conjecture rived from and enforced by any body else's conjecture, that by his knowledge and his testimony.

§ 10. There is a class of cases in apparent opposition to me of the statements in this chapter, but which will be und, when examined closely, decidedly to confirm them. I am walking, say, in a remote part of the country, and sud-enly meet with a friend. At this I am naturally surprised. et if the view be correct that we cannot properly speak bout events in themselves being probable or improbable, ut only say this of our conjectures about them, how do we plain this? We had formed no conjecture beforehand, r we were not thinking about anything of the kind, but et few would fail to feel surprise at such an incident.

The reply might fairly be made that we *had* formed ch anticipations tacitly. On any such occasion every ne unconsciously divides things into those which are known o him and those which are not. During a considerable previous period a countless number of persons had met and all fallen into the list of the unknown to us. T was nothing to remind us of having formed the antic tion or distinction at all, until it was suddenly called into vivid consciousness by the exceptional event. words which we should instinctively use in our surprise s to show this:—'Who would have thought of seeing here?' viz. Who would have given any weight to the la thought if it had been called out into consciousness bef hand? We put our words into the past tense, showing we have had the distinction lurking in our minds all time. We always have a multitude of such ready-m classes of events in our minds, and when a thing happen fall into one of those classes which are very small we ca help noticing the fact.

Or suppose I am one of a regiment into which a flies, and it strikes me, and me only. At this I am prised, and why? Our common language will guide us to reason. 'How strange that it should just have hit *me* o men!' We are thinking of the very natural two-fold divi of mankind into, ourselves, and everybody else; our sur is again, as it were, retrospective, and in reference to division. No anticipation was distinctly formed, bec we did not think beforehand of the event, but the ev when it has happened, is at once assigned to its approp class.

§ 11. This view is confirmed by the following consid tions. Tell the story to a friend, and he will be a l surprised, but less so than we were, *his* division in particular case being,—his friends (of whom we are but and the rest of mankind. It is not a necessary division it is the one which will be most likely suggested to him.

Tell it again to a perfect stranger, and his division b ferent (viz. we falling into the majority) we shall fail to ke him perceive that there is anything at all remarkable the event.

It is not of course attempted in these remarks to justify r surprise in every case in which it exists. Different sons might be differently affected in the cases supposed, d the examples are therefore given mainly for illustration. ill on principles already discussed (Ch. VI. § 32) we might pect to find something like a general justification of the ount of surprise.

§ 12. The answer commonly given in these cases is fined to attempting to show that the surprise should not se, rather than to explaining how it does arise. It takes e following form,—'You have no right to be surprised, for thing remarkable has really occurred. If this particular ing had not happened something equally improbable ust. If the shot had not hit you or your friend, it must ve hit some one else who was *à priori* as unlikely to be t.'

For one thing this answer does not explain the fact t almost every one *is* surprised in such cases, and surised somewhat in the different proportions mentioned ove. Moreover it has the inherent unsatisfactoriness admitting that something improbable has really hapned, but getting over the difficulty by saying that all the her alternatives were equally improbable. A natural inence from this is that there is a class of things, in themves really improbable, which can yet be established upon ry slight evidence. Butler accepted this inference, and rked it out to the strange conclusion given above. Mill tempts to avoid it by the consideration of the very different values to be assigned to improbability before and after e event. Some further discussion of this point will be found in the chapter on Fallacies, and in that on the Cr bility of Extraordinary Stories.

§ 13. In connection with the subject at present un discussion we will now take notice of a distinction which shall often find insisted on in works on Probability, but which apparently needless importance has been attach It is frequently said that probability is *relative*, in the se that it has a different value to different persons accordi to their respective information upon the subject in qu tion. For example, two persons, *A* and *B*, are going to dr a ball from a bag containing 4 balls: *A* knows that balls are black and white, but does not know mo *B* knows that three are black and one white. It wo be said that the probability of a white ball to *A* is ½, to *B* ¼.

When however we regard the subject from the mate standing point, there really does not seem to me much m in this than the principle, equally true in every other scien that our inferences will vary according to the data we sume. We might on logical grounds with almost equ propriety speak of the area of a field or the height of mountain being relative, and therefore having one value one person and another to another. The real meaning of t example cited above is this: *A* supposes that he is choosi white at random out of a series which in the long run wo give white and black equally often; *B* supposes that is choosing white out of a series which in the long run wo give three black to one white. By the application, the fore, of a precisely similar rule they draw different concl sions; but so they would under the same circumstances any other science. If two men are measuring the height a mountain, and one supposes his base to be 1000 fe whilst the other takes it to be 1001, they would of cou form different opinions about the height. The science of mensuration is not supposed to have anything to do with the truth of the data, but assumes them to have been correctly taken; why should not this be equally the case with Probability, making of course due allowance for the peculiar character of the data with which it is concerned?

§ 14. This view of the relativeness of probability is connected, as it appears to me, with the subjective view of the science, and is indeed characteristic of it. It seems a fair illustration of the weak side of that view, that it should lead us to lay any stress on such an expression. As was fully explained in the last chapter, in proportion as we work out the Conceptualist principle we are led away from the fundamental question of the material logic, viz. Is our belief actually correct, or not? and, if the former, to what extent and degree is it correct? We are directed rather to ask, What belief does any one as a matter of fact hold? And, since the belief thus entertained naturally varies according to the circumstances and other sources of information of the person in question, its relativeness comes to be admitted as inevitable, or at least it is not to be wondered at if such should be the case.

On our view of Probability, therefore, its 'relativeness' in any given case is a misleading expression, and it will be found much preferable to speak of the effect produced by variations in the nature and amount of the data which we have before us. Now it must be admitted that there are frequently cases in our science in which such variations are peculiarly likely to be found. For instance, I am expecting a friend who is a passenger in an ocean steamer. There are a hundred passengers on board, and the crew also numbers a hundred. I read in the papers that one person was lost by falling overboard; my anticipation that it was my friend who was lost is but small, of course. On turning to another paper, I see that the man who was lost was a passenger, not one of the crew; my slight anxiety is at once doubled. But another account adds that it was an Englishman, and on that line at that season the English passengers are known to be few; I at once begin to entertain decided fears. And so on, every trifling bit of information instantly affecting my expectations.

§ 15. Now since it is peculiarly characteristic of Probability, as distinguished from Induction, to be thus at the mercy, so to say, of every little fact that may be floating about when we are in the act of forming our opinion, what can be the harm (it may be urged) of expressing this state of things by terming our state of expectation *relative?*

There seem to me to be two objections. In the first place, as just mentioned, we are induced to reject such an expression on grounds of consistency. It is inconsistent with the general spirit and treatment of the subject hitherto adopted, and tends to divorce Probability from Inductive logic instead of regarding them as cognate sciences. We are aiming at truth, as far as that goal can be reached by our road, and therefore we dislike to regard our conclusions as relative in any other sense than that in which truth itself may be said to be relative.

In the second place, this condition of unstable assent, this constant liability to have our judgment affected, to any degree and at any moment, by the accession of new knowledge, though doubtless characteristic of Probability, does not seem to me characteristic of it in its sounder and more legitimate applications. It seems rather appropriate to a precipitate judgment formed in accordance with the rules than a strict example of their natural employment. Such precipitate judgments may occur in the case of ordinary de uctive conclusions. In the practical exigencies of life we re constantly in the habit of forming a hasty opinion with early full confidence, at any rate temporarily, upon the trength of evidence which we must well know at the time annot be final. We wait a short time, and something else urns up which induces us to alter our opinion, perhaps to reverse it. Here our conclusions may have been perfectly ound under the given circumstances, that is, they may be uch as every one else would have drawn who was bound to make up his mind upon the data before us, and they are nquestionably 'relative' judgments in the sense now under discussion. And yet, I think, every one would shrink from o terming them who wished systematically to carry out the view that Logic was to be regarded as an organon of truth.

§ 16. In the examples of Probability which we have hitherto employed, we have for the most part assumed that here was a certain body of statistics set before us on which our conclusion was to rest. It was assumed, on the one hand, that no direct specific evidence could be got, so that the judgment was really to be one of Probability, and to rest n these statistics; in other words, that nothing better than hem was available for us. But it was equally assumed, on he other hand, that these statistics were open to the observation of every one, so that we need not have to put up with nything inferior to them in forming our opinion. In other words, we have been assuming that here, as in the case of most other sciences, those who have to draw a conclusion tart from the same footing of opportunity and information. This, for instance, clearly is or ought to be the case when we are concerned with games of chance; ignorance or misapprehension of the common data is never contemplated here. So with the statistics of life, or other insurance: so ong as our judgment is to be accurate (after its fashion) or justifiable, the common tables of mortality are all that an one has to go by.

§ 17. It is true that in the case of a man's prospect o death we should each qualify our judgment by what w knew or reasonably supposed as to his health, habits, profession, and so on, and should thus arrive at varying estimates. But no one could *justify* his own estimate withou appealing explicitly or implicitly to the statistical grounds o which he had relied, and if these were not previously avail able to other persons, he must now set them before the notice. In other words, the judgments we entertain, here elsewhere, are only relative so long as we rest them o grounds peculiar to ourselves. The process of justificatio which I consider to be essential to logic, has a tendency t correct such individualities of judgment, and to set all ob servers on the same basis as regards their data.

It is better therefore to regard the conclusions of Prob bility as being absolute and objective, in the same sense as though doubtless in a far less degree than, they are in Induc tion. Fully admitting that our conclusions will in man cases vary exceedingly from time to time by fresh accessio of knowledge, it is preferable to regard such fluctuations o assent as partaking of the nature of precipitate judgment founded on special statistics, instead of depending only o those which are common to all observers. In calling suc judgments precipitate it is not implied that there is an blame in entertaining them, but simply that, for one reaso or another, we have been induced to form them withou waiting for the possession of the full amount of evidence statistical or otherwise, which might ultimately be looked for. This explanation will suit the facts equally well, and is more consistent with the general philosophical position maintained in this work.

CHAPTER XIII.

ON THE CONCEPTION AND TREATMENT OF MODALITY.

1. THE reader who knows anything of the scholastic ogic will have perceived before now that we have been uching in a variety of places upon that most thorny and pulsive of districts in the logical territory;—modality. It ill be advisable, however, to put together, somewhat more efinitely, what has to be said upon the subject. I propose, herefore, to devote this chapter to a brief account of the rincipal varieties of treatment which the modals have reeived at the hands of professed logicians.

It must be remarked at the outset that the sense in hich modality and modal propositions have been at various imes understood, is by no means fixed and invariably the ame. This diversity of view has arisen partly from coresponding differences in the view taken of the province and ature of logic, and partly from differences in the philoophical and scientific opinions entertained as to the contitution and order of nature. In later times, moreover, nother very powerful agent in bringing about a change in he treatment of the subject must be recognized in the radual and steady growth of the theory of Probability, as orked out by the mathematicians from their own point of iew.

§ 2. In spite, however, of these differences of treatment, there has always been some community of subject-matter in the discussions upon this topic. There has almost always been some reference to quantity of belief; enough perl to justify De Morgan's[1] remark, that Probability was " unknown God whom the schoolmen ignorantly worshir when they so dealt with this species of enunciation, tha was said to be beyond human determination whether t most tortured the modals, or the modals them." But reference to quantity of belief has sometimes been direct immediate, sometimes indirect and arising out of the nat of the subject-matter of the proposition. The fact is, t that distinction between the purely subjective and purely jective views of logic, which I have endeavoured to bring into prominence in the eleventh chapter, was not by means clearly recognized in early times, nor indeed be the time of Kant, and the view to be taken of moda naturally shared in the consequent confusion. This wi hope, be made clear in the course of the following cha which is intended to give a brief sketch of the princ different ways in which the modality of propositions been treated in logic. As it is not proposed to give thing like a regular history of the subject, there will b necessity to adhere to any strict sequence of time, o discuss the opinions of any writers, except those who ma taken as representative of tolerably distinct views. outcome of such investigation will be, I hope, to conv the reader (if, indeed, he had not come to that convic before), that the logicians, after having had a long and trial, have failed to make anything satisfactory out of subject of the modals by their methods of enquiry and tr ment; and that it ought, therefore, to be banished enti from that science, and relegated to Probability.

§ 3. From the earliest study of the syllogistic pro it was seen that, complete as that process is within its omain, the domain, at any rate under its simplest treatment, s a very limited one. Propositions of the pure form,—All or some) *A* is (or is not) *B*,—are found in practice to form ut a small portion even of our categorical statements. We are perpetually meeting with others which express the relation of *B* to *A* with various degrees of necessity or probability; e.g. *A* must be *B*, *A* may be *B*; or the effect of such facts upon our judgment, e.g. I am perfectly certain that *A* is *B*, I think that *A* may be *B*; with many others of a more or less similar type. The question at once arises, How are such propositions to be treated? It does not seem to have occurred to the old logicians, as to some of their successors in modern times, simply to reject all consideration of this topic. Their faith in the truth and completeness of their system of inference was far too firm for them to suppose it possible that forms of proposition universally recognized as significant in popular speech, and forms of inference universally recognized there as valid, were to be omitted because they were inconvenient or complicated.

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[1] *Formal Logic*, p. 232.

§ 4. One very simple plan suggests itself, and has indeed been repeatedly advocated, viz. just to transfer all that is characteristic of such propositions into that convenient receptacle for what is troublesome elsewhere, the predicate[1]. Has not another so-called modality been thus got rid of[2]?

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[1] This appears to be the purport of some statements in a very confused passage in Whately's *Logic* (Bk. II., ch. IV. § 1). "A modal proposition may be stated as a pure one by attaching the mode to one of the terms, and the proposition will in all respects fall under the foregoing rules;...'It is probable that all knowledge is useful;' 'probably useful' is here the predicate." He draws apparently no such distinction as that between the true and false modality referred to in the next note. What is really surprising is that even Hamilton puts the two (the true and the false modality) upon the same footing. "In regard to these [the former] the case is precisely the same; the mode is merely a part of the predicate." *Logic*, I. 257.

[2] I allude of course to such ex-

and has it not been attempted by the same device to al the distinctive characteristic of negative propositions, v shifting the negative particle into the predicate? It be admitted that, up to a certain point, something m done in this way. Given the reasoning, 'Those who arsenic will probably die; *A* has taken it, therefore h probably die;' it is easy to convert this into an ord syllogism of the pure type, by simply wording the n 'Those who take arsenic are people-who-will-probabl when the conclusion follows in the same form, '*A* i who-will-probably-die.' But this device will only car a very little way. Suppose that the minor premise a of the same modal description, e.g. '*A* has probably arsenic,' and it will be seen that we cannot relegat modality here also to the predicate without being broug a stop by finding that there are four terms in the syllog

But even if there were not this particular objecti does not appear that anything is to be gained in the v intelligibility or method by such a device as the above.

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amples as '*A* killed *B* unjustly,' in which the killing of *B* by *A* was sometimes said to be asserted not simply but with a modification. (Hamilton's *Logic*, I. 256.) It is obvious that the modification in such cases is by rights merely a part of the predicate, there being no formal distinction between '*A* is the killer of *B*' and '*A* is the unjust killer of *B*.' Indeed some logicians who were too conservative to reject the generic name of modality in this application adopted the common expedient of introducing a specific distinction which did away with its *meaning*, terming the spurious kind 'material modality' and the g kind 'formal modality'. The included all the cases in whi modification belonged by righ to the predicate or to the s the latter was reserved for th in which the modification the real conjunction of the p with the subject. (Kecke *Systema Logicæ*, Lib. II. ch. was, I believe, a common sc distinction.

For some account of the as to whether the negative was to be considered to be the copula or to the predic Hamilton's *Logic*, I. 253.

at is meant by a modal predicate, by the predicate
obably mortal,' for instance, in the proposition 'All poison-
gs by arsenic are probably mortal'? If the analogy with
dinary pure propositions is to hold good, it must be a
edicate referring to the *whole* of the subject, for the sub-
ct is distributed. But then we are at once launched into
e difficulties discussed in a former chapter (Ch. VI. §§ 19—
), when we attempt to justify or verify the application of
e predicate. We have to enquire (at least on the view
opted in this work) whether the application of the pre-
ate 'probably mortal' to the *whole* of the subject, really
ans at bottom anything else than that the predicate
ortal' is to be applied to a *portion* (more than half) of the
mbers denoted by the subject. When the transference of
e modality to the predicate raises such intricate questions
to the sense in which the predicate is to be interpreted,
ere is surely nothing gained by the step.

§ 5. A second, and more summary way of shelving all
ficulties of the subject, so far at least as logic, or the
iters upon logic, are concerned, is found by simply denying
t modality has any connection whatever with logic. This
the course adopted by many modern writers, for instance,
Hamilton and Mansel, in reference to whom one cannot
p remarking that an unduly large portion of their logical
itings seems occupied with telling us what does *not* belong
logic. They justify their rejection on the ground that
e mode belongs to the matter, and must be determined by
consideration of the matter, and therefore is extralogical.
a certain extent I agree with their grounds of rejection,
(as explained in Chapter VI.) it is not easy to see how
e degree of modality of any proposition, whether premise
conclusion, can be justified without appeal to the matter.
t then questions of justification, in any adequate sense of

the term, belong to a range of considerations somewhat to Hamilton's and Mansel's way of regarding the s The complete justification of our inferences is a which involves their truth or falsehood, a point with these writers do not much concern themselves, being occupied with the consistency of our reasonings, not their conformity with fact. Were I speaking as a tonian I should say that modality *is* formal rather material, for though we cannot justify the degree belief of a proposition without appeal to the matter, to a moderate degree of accuracy estimate it withou such appeal; and this would seem to be quite enou warrant its being regarded as formal.

It must be admitted that Hamilton's account matter when he is recommending the rejection of the is not by any means clear and consistent. He not onl as already remarked, to distinguish between the form the material (in other words, the true and the false dality; but when treating of the former he fails to disti between the extremely diverse aspects of modality viewed from the Aristotelian and the Kantian stand- Of the amount and significance of this difference we speak presently, but it may be just pointed out her Hamilton begins (Vol. I. p. 257) by rejecting the mod the ground that the distinctions between the necessa contingent, the possible, and the impossible, must be rested on an appeal to the matter of the propositio which he is, I think, quite correct. But then a little f on (p. 260), in explaining 'the meaning of three terms are used in relation to pure and modal propositions,' he the widely different Kantian, or threefold division in apodeictic, the assertory, and the problematic. He do take the precaution of pointing out to his hearers th

; general views of logic from which these two accounts lity spring[1].

There is one kind of modal syllogism which it ;em unreasonable to reject on the ground of its not ›rmal, and which we may notice in passing. The 'Any *A* is probably *B*,' is equivalent to 'Most *A* are w it is obvious that from two such premises as 'Most ?,' 'Most *A* are *C*,' we can deduce the consequence, *C* are *B*.' Since this holds good whatever may be ıre of *A*, *B*, and *C*, it is, according to ordinary usage erm, a formal syllogism. Mansel, however, refuses to ıat any such syllogisms belong to formal logic. His are given in a rather elaborate review[2] and criticism of the logical works of De Morgan, to whom the :tion of 'numerically definite syllogisms' is mainly [ansel does not take the particular example given ıs he is discussing a somewhat more comprehensive c form. He examines it in a special numerical :[3]:—18 out of 21 *Y*s are *X*; 15 out of 21 *Y*s are *Z*; :lusion that 12 *Z*s are *X* is rejected from formal logic ground that the arithmetical judgment involved is .cal, not analytical, and rests upon an intuition of /. We cannot enter upon any examination of these

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has also given a short . of the subject elsewhere *ns*, Ed. II. p. 702), in which at different view is taken. s are indeed here admitted but only in so far as they division under the relation and species, which is of ıtamount to their entire for they then differ in no ay from any other exam- t relation.

[2] *Letters, Lectures and Reviews*, p. 61. Elsewhere in the review (p. 45) he gives what appears to me a somewhat different decision.

[3] It must be remembered that this is not one of the proportional propositions with which we have been concerned in previous chapters: it is meant that there are *exactly* 21 *Y*s, of which just 18 are *X*, not that on the average 18 out of 21 may be so regarded.

reasons here; but it may merely be remarked th criticism demands the acceptance of the Kantian do as to the nature of arithmetical judgments, and that it be better to base the rejection not on the ground th syllogism is not *formal*, but on the ground that it *analytical.*

§ 7. There is another and practical way of getti of the perplexities of modal reasoning which must be here. It is the resource of ordinary reasoners rather th decision of professed logicians[1], and, like the first met evasion already pointed out in this chapter, is of very application. It consists in treating the premises, duri process of reasoning, as if they were pure, and th introducing the modality into the conclusion, as a qualification of its full certainty. When each of th mises is nearly certain, or when from any cause we a concerned with the extent of their departure from fu tainty, this rough expedient will answer well enough. I apprehend, the process which passes through the m most persons in such cases, in so far as they reason cons They would, presumably, in such an example as tha viously given (§ 4), proceed as if the premises that who take arsenic will die,' and that 'the man in q has taken it,' were quite true, instead of being only pr true, and they would consequently draw the conclusi 'he would die.' But bearing in mind that the premi not certain, they would remember that the conclusi only to be held with a qualified assent. This they press quite correctly, if the mere nature and not the gree of that assent is taken into account, by saying that e is likely to die.' In this case the modality is rejected mporarily from the premises to be reintroduced into the nclusion.

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[1] I consider however, as I have said further on (p. 320), that the treatment in the older logics of Probable syllogisms, and Dialectic syllogisms, *came to* somewhat the same thing as this, though they looked at ter from a different point and expressed it in very language.

It is obvious that such a process as this is of a very ugh and imperfect kind. It does, in fact, omit from accuate consideration just the one point now under discussion. t takes no account of the varying shades of expression by hich the degree of departure from perfect conviction is dicated, which is of course the very thing with which odality is intended to occupy itself. At best, therefore, it uld only claim to be an extremely rude way of deciding uestions, the accurate and scientific methods of treating hich are demanded of us.

§ 8. In any employment of applied logic we have of urse to go through such a process as that just mentioned. utside of pure mathematics it can hardly ever be the case at the premises from which we reason are held with absoute conviction. Hence there must be a lapse from absolute nviction in the conclusion. But we reason on the hypoesis that the premises are true, and any trifling defection om certainty, of which we may be conscious, is mentally served as a qualification to the conclusion. But such coniderations as these belong rather to ordinary applied logic; ey amount to nothing more than a caution or hint to be rne in mind when the rules of the syllogism, or of inction, are applied in practice. When, however, we are eating of modality, the extent of the defection from full rtainty is supposed to be sufficiently great for our language indicate and appreciate it. What we then want is of urse a scientific discussion of the principles in accordance ith which this departure is to be measured and expressed, both in our premises and in our conclusion. Such a pl therefore for treating modality, as the one under discussio is just as much a banishment of it from the field of re logical enquiry, as if we had determined avowedly to reject from consideration.

§ 9. Before proceeding to a discussion of the vario ways in which modality may be treated by those who adm it into logic, something must be said to clear up a pos ble source of confusion in this part of the subject. In th cases with which we have hitherto been mostly concerne in the earlier chapters of this work, the characteristic modality (for in this chapter we may with propriety use th logical term) has generally been found in singular and pa ticular propositions. It presented itself when we had judge of individual cases from a knowledge of the averag and was an expression of the fact that the proposition re lating to these individuals referred to a portion only of th whole class from which the average was taken. Given th of men of fifty-five, three out of five will die in the course twenty years, we have had to do with propositions of th vague form, 'It is probable that *AB* (of that age) will die or of the more precise form, 'It is three to two that *AB* wi die,' within the specified time. Here the modal propositio naturally presents itself in the form of a singular or pa ticular proposition.

§ 10. But when we turn to ordinary logic we may fi *universal* propositions spoken of as modal. This must most be the case with those which are termed necessary or im possible, but it may also be the case with the probable. W may meet with the form 'All *X* is probably *Y*.' Adoptin the same explanation here as has been throughout adopted i analogous cases, we must say that what is meant by th modality of such a proposition is the proportional number ıes in which the universal proposition would be correctly ıde. And in this there is, so far, no difficulty. The only fference is that whereas the justification of the former, viz. e particular or individual kind of modal, was obtainable thin the limits of the universal proposition which included the justification of the modality of a universal proposition ıs to be sought in a group or succession of other propoıions. The proposition has to be referred to some group of nilar ones and we have to consider the proportion of cases which it will be true. But this distinction is not at all ndamental.

It is quite true that universal propositions from their ıture are much less likely than individual ones to be justiıd, in practice, by such appeal. But, as has been already quently pointed out, we are not concerned with the way which our propositions are practically obtained, nor with e way in which men might find it most natural to test em; but with that ultimate justification to which we apal in the last resort, and which has been abundantly shown be of a statistical character. When, therefore, we say that is probable that all *X* is *Y*,' what we mean is, that in ɔre than half the cases we come across we should be right so judging, and in less than half the cases we should be ong.

§ 11. It is at this step that the possible ambiguity is countered. When we talk of the chance that All *X* is *Y*, contemplate or imply the complementary chance that it is t so. Now this latter alternative is not free from amuity. It might happen, for instance, in the cases of failure, ıt no *X* is *Y*, or it might happen that some *X*, only, is not for both of these suppositions contradict the original proition, and are therefore instances of its failure. In prac-, no doubt, we should have various recognized rules and inductions to fall back upon in order to decide between alternatives, though, of course, the appeal to them wou in strictness extralogical. But the mere existence of su ambiguity, and the fact that it can only be cleared appeal to the subject-matter, are in themselves no difficulty in the application of the conception of modali universal propositions as well as to individual ones.

§ 12. Having noticed some of the ways in whicl introduction of modality into logic has been evaded jected, we must now enter into a brief account of its ment by those who have more or less deliberately adm its claims to acceptance.

The first enquiry will be, What opinions have been as to the nature of modality? that is, Is it primarily a fection of the matter of the proposition, and, if not, what exactly? In reference to this enquiry it appears to already remarked, that amongst the earlier logicians no clear and consistent distinction between the subjective objective views of logic as is now commonly maintained be detected[1]. The result of this appears in their treat of modality. This always had some reference to the jective side of the proposition, viz. in this case to the n or quantity of the belief with which it was entertained it is equally clear that this characteristic was not estin at first hand, so to say, and in itself, but rather from a sideration of the matter determining what it should be. commonly accepted scholastic or Aristotelian division instance, is into the necessary, the contingent, the pos and the impossible. This is clearly a division accordi matter almost entirely, for on the purely mental side the cessary and the impossible would be just the same; one plying full conviction of the truth of a proposition, and the ner of that of its contradictory. So too, on the same side, it uld not be easy to distinguish between the contingent and e possible. On the view in question, therefore, the modality a proposition was determined by a reference to the nature the subject-matter. In some propositions the nature of e subject-matter decided that the predicate was necessarily ined to the subject; in others that it was impossible that ey should be joined; and so on.

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[1] The distinction is however by no means entirely neglected. Thus Smiglecius, when discussing the *modal affections* of certainty and necessity, says, "certitudo ad cognit spectat: necessitas vero est i (*Disputationes;* Disp. XIII., XII.).

§ 13. The artificial character of such a four-fold division ill be too obvious to modern minds for it to be necessary to iticize it. A very slight study of nature and consequent preciation of inductive evidence suffice to convince us that ose uniformities upon which all connections of phenomena, hether called necessary or contingent, depend, demand extremely profound and extensive enquiry; that they admit of such simple division into clearly marked groups; and at, therefore, the pure logician had better not meddle with em[1].

The following extract from Grote's *Aristotle* (Vol. ɪ. p. 2) will serve to show the origin of this four-fold division, conformity with the science of the day, and consequently utter want of conformity with that of our own time:— The distinction of Problematical and Necessary Propositions rresponds, in the mind of Aristotle, to that capital and aracteristic doctrine of his Ontology and Physics, already uched on in this chapter. He thought, as we have seen, at in the vast circumferential region of the Kosmos, from the outer sidereal sphere down to the lunar sphere, cel substance was a necessary existence and energy, sempit and uniform in its rotations and influence; and that thr its beneficent influence, pervading the concavity betwee lunar sphere and the terrestrial centre (which included four elements with their compounds) there prevailed a r larizing tendency called Nature; modified, however, partly counteracted by independent and irregular forces c Spontaneity and Chance, essentially unknowable and u dictable. The irregular sequences thus named by Aris were the objective correlate of the Problematical Propos in Logic. In these sublunary sequences, as to future t *may or may not*, was all that could be attained, even b highest knowledge; certainty, either of affirmation or n tion, was out of the question. On the other hand, the n sary and uniform energies of the celestial substance, fo the objective correlate of the Necessary Proposition in L this substance was not merely an existence, but an exist necessary and unchangeable...he considers the Problema Proposition in Logic to be not purely subjective, a expression of the speaker's ignorance, but something n namely, to correlate with an objective essentially unknow to all."

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[1] It may be remarked that Whately (*Logic*, Bk. ɪɪ. ch. ɪɪ. § 2) speaks necessary, impossible and contingent matter, without any apparent suspicion that they belong entirely *to* an obsolete point of view.

§ 14. Even after this philosophy began to pass a the divisions of modality originally founded upon it m have proved, as De Morgan has remarked[1], of conside service in mediæval times. As he says, people were n more frequently required to decide in one way or the o upon a single testimony, without there being a sufficien specific knowledge to test the statements made. The logician "did not know but that any day of the week n bring from Cathay or Tartary an account of men who r vheels of flesh and blood, or grew planted in the ground, ?olydorus in the Æneid, as well evidenced as a great nearly as marvellous stories." Hence, in default of r inductions, it might have been convenient to make ı classifications of the facts which were and which were o be accepted on testimony (the necessary, the impos- &c.), and to employ these provisional inductions (which we should now regard them) as testing the stories ı reached him. Propositions belonging to the class of mpossible might be regarded as having an antecedent mption against them so great as to prevail over almost estimony worth taking account of, and so on.

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[1] *Formal Logic*, p. 233.

15. But this old four-fold division of modals con- d to be accepted and perpetuated by the logicians long all philosophical justification for it had passed away. r as I have been able to ascertain, scarcely any logician pute or popularity before Kant, was bold enough to any important change in the way of regarding them[1]. the Port-Royal Logic, founded as it is on Cartesianism, ts the traditional statements, though with extreme ty. This adherence to the old forms led, it need not be rked, to considerable inconsistency and confusion in cases. These forms were founded, as we have seen, on jective view of the province of logic, and this view was

ıe subject was sometimes al- ər omitted, as by Wolf. He good deal however about pro- propositions and syllogisms, ke Leibnitz before him, looked d to a "logica probabilium" ıething new and desirable. I e that he had been influenced writers on Chances, as of the ıo had already treated that nearly all the most important are referred to in one passage (*Philosophia Rationalis sive Logica*, § 593).

Lambert stands quite apart. In this respect, as in most others where mathematical conceptions and symbols are involved, his logical attitude is thoroughly unconventional. See, for instance, his chapter 'Von dem Wahrscheinlichen', in his *Neues Organon*.

by no means rigidly carried out in many cases. In fact
was beginning to be abandoned, to an extent and in dir
tions which we have not opportunity here to discuss, bef
the influence of Kant was felt. Many, for instance, added
the list of the four, by including the true and the fal
occasionally also the probable, the supposed, and the cert
were added. This seems to show some tendency tow
abandoning the objective for the subjective view, or at le
indicates a hesitation between them.

§ 16. With Kant's view of modality almost every one
familiar. He divides judgments, under this head, into t
apodeictic, the assertory, and the problematic. We shall h
to say something about the number and mutual relations
these divisions presently; we are now only concerned wi
the general view which they carry out. In this respect it w
be obvious at once what a complete change of position h
been reached. The 'necessary' and the 'impossible' d
manded an appeal to the matter of a proposition in order
recognize them; the 'apodeictic' and the 'assertory', on t
other hand, may be true of almost any matter, for th
demand nothing but an appeal to our consciousness in ord
to distinguish between them. Moreover, the distinctio
between the assertory and the problematic is so entir
subjective and personal, that it may vary not only betwe
one person and another, but in the case of the same pers
at different times. What one man knows to be true, anoth
may happen to be in doubt about. The apodeictic judgme
is one which we not only accept, but which we find oursel
unable to reverse in thought; the assertory is simply ac
cepted; the problematic is one about which we feel i
doubt.

This way of looking at the matter is the necessary out
come of the conceptualist or Kantian view of logic. It h

een followed by many logicians, not only by those who may
e called followers of Kant, but by almost all who have felt
is influence. Ueberweg, for instance, who is altogether at
sue with Kant on some fundamental points, adopts it.

§ 17. The next question to be discussed is, How many
ubdivisions of modality are to be recognized? The Aristo-
elian or scholastic logicians, as we have seen, adopted a four-
ld division. The exact relations of some of these to each
ther, especially the possible and the contingent, is an ex-
remely obscure point, and one about which the commenta-
ors are by no means agreed. As, however, it seems tolerably
lear that it was not consciously intended by the use of these
ur terms to exhibit a graduated scale of intensity of con-
ction, their correspondence with the province of modern
obability is but slight, and the discussion of them, there-
re, becomes rather a matter of special or antiquarian in-
erest. De Morgan, indeed (*Formal Logic*, p. 232), says that
he schoolmen understood by contingent more likely than
ot, and by possible less likely than not. I do not know
n what authority this statement rests, but it credits them
ith a much nearer approach to the modern views of proba-
ility than one would have expected, and decidedly nearer than
at of most of their successors[1]. The general conclusion at
hich I have arrived, after a reasonable amount of investiga-
on, is that there were two prevalent views on the subject.
ome (e.g. Burgersdyck, Bk. I. ch. 32) admitted that there
ere at bottom only two kinds of modality; the contingent
d the possible being equipollent, as also the necessary and
e impossible, provided the one asserts and the other
nies. This is the view to which those would naturally
led who looked mainly to the nature of the subject-matter.

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[1] I cannot find the slightest authority for the statement in the
borate history of Logic by Prantl.

On the other hand, those who looked mainly at the for expression, would be led by the analogy of the four forn proposition, and the necessity that each of them should s in definite opposition to each other, to insist upon a dis tion between the four modals[1]. They, therefore, endeavo to introduce a distinction by maintaining (e.g. Crackanth Bk. III. ch. 11) that the contingent is that which now is may not be, and the possible that which now is not but be. A few appear to have made the distinction c spondent to that between the physically and the logi possible.

§ 18. When we get to the Kantian division we reached much clearer ground. The meaning of each of t terms is quite explicit, and it is also beyond doubt that have a more definite tendency in the direction of assigni graduated scale of conviction. So long as they are rega from a metaphysical rather than a logical standing p there is much to be said in their favour. If we use i spection merely, confining ourselves to a study of the j ments themselves, to the exclusion of the grounds on w they rest, there certainly does seem a clear and well-ma distinction between judgments which we cannot even ceive to be reversed in thought; those which we c reverse, but which we accept as true; and those whicl merely entertain as possible.

Regarded, however, as a logical division, Kant's arra ment seems to me of very little service. For such lo purposes indeed, as we are now concerned with, it r seems to resolve itself into a *two*-fold division. The tinction between the apodeictic and the assertory wi itted, I presume, even by those who accept the metasical or psychological theory upon which it rests, to be a erence which concerns, not the quantity of belief with ch the judgments are entertained, but rather the violence ch would have to be done to the mind by the attempt to et them. Each is fully believed, but the one can, and other cannot, be controverted. The belief with which an ertory judgment is entertained is full belief, else it would differ from the problematic; and therefore in regard to quantity of belief, as distinguished from the quality or racter of it, there is no difference between it and the apotic. It is as though, to offer an illustration, the index been already moved to the top of the scale in the asory judgment, and all that was done to convert this into apodeictic one, was to *clamp* it there. The only logical erence which then remains is that between problematic assertory, the former comprehending all the judgments to the truth of which we have any degree of doubt, and latter those of which we have no doubt. The whole ge of the former, therefore, with which Probability is ropriately occupied, is thrown undivided into a single partment. We can hardly speak of a 'division' where class includes everything up to the boundary line, and other is confined to that boundary line. Practically, refore, on this view, modality, as the mathematical stut of Probability would expect to find it, as completely ppears as if it were intended to reject it.

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[1] "Hi quatuor modi magnam censeri solent analogiam habere cum *quadruplici* propositionum in quantitate et qualitate varietate" (W *Instit. Logic.* Bk. II. ch. 8).

§ 19. By less consistent and systematic thinkers, and those in whom ingenuity was an over prominent feature, riety of other arrangements have been accepted or pro-d. There is, of course, some justification for such attempts he laudable desire to bring our logical forms into better nony with ordinary thought and language. In practice, as was pointed out in an earlier chapter, every one re nizes a great variety of modal forms, such as 'likely,' ' likely,' 'almost certainly,' and so on almost without limi each direction. It was doubtless supposed that, by negl ing to make use of technical equivalents for some of t forms, we should lose our logical control over certain pos kinds of inference, and so far fall short even of the preci of ordinary thought.

With regard to such additional forms, it appears to that all those which have been introduced by writers were uninfluenced by the Theory of Probability, have little else than create additional confusion, as such writer not attempt to marshal their terms in order, or to ascer their mutual relations. Omitting, of course, forms obvio of material modality, we have already mentioned the and the false; the probable, the supposed, and the cer These subdivisions seem to have reached their climax very early stage in Occam (Prantl, III. 380), who held th proposition might be modally affected by being 'vera, s falsa, ignota, scripta, prolata, concepta, credita, opinata, bitata.'

§ 20. Since the growth of the science of Probabi logicians have had better opportunities of knowing they had to aim at; and, though it cannot be said that t attempts have been really successful, these are at any ra decided improvement upon those of their predecessors. Thomson[1], for instance, gives a nine-fold division. He that, arranging the degrees of modality in an ascen scale, we find that a judgment may be either poss doubtful, probable, morally certain for the thinker him morally certain for a class or school, morally certain for physically certain with a limit, physically certain wit

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[1] *Laws of Thought*, § 118.

.tation, and mathematically certain. Many other divi- s might doubtless be mentioned, but, as every mathema- ın will recognize, the attempt to secure any general ement in such a matter of arrangement is quite hopeless. s here that the beneficial influence of the mathematical ɔry of Probability is to be gratefully acknowledged. As ı as this came to be studied it must have been perceived t in attempting to mark off clearly from one another ain gradations of belief, we should be seeking for breaches ı continuous magnitude. In the advance from a slight ımption to a strong presumption, and from that to moral ainty, we are making a gradual ascent, in the course of ch there are no natural halting-places. The proof of this tinuity need not be entered upon here, for the materials it will have been gathered from almost every chapter of work. The reader need merely be reminded that the ınds of our belief, in all cases which admit of number and ısurement, are clearly seen to be of this description; and t therefore unless the belief itself is to be divorced from grounds on which it rests, what thus holds as to their racteristics must hold also as to its own.

It follows, therefore, that modality in the old sense of the d, wherein an attempt was made to obtain certain natural sions in the scale of conviction, must be finally abandoned. that it endeavoured to do can now be done incomparably ter by the theory of Probability, with its numerical scale ch admits of indefinite subdivision. None of the old sys- s of division can be regarded as a really natural one; se which admit but few divisions being found to leave the le range of the probable in one unbroken class, and those ch adopt many divisions lapsing into unavoidable vague- ı and uncertainty.

§ 21. Corresponding to the distinction between pure and modal propositions, but even more complicated unsatisfactory in its treatment, was that between pur modal syllogisms. The thing discussed in the case o latter was, of course, the effect produced upon the concl in respect of modality, by the modal affection of one or premises. It is only when we reach such consideratio these that we are at all getting on to the ground appro to Probability; but it is obvious that very little cou done with such rude materials, and the inherent clum and complication of the whole modal system come out clearly here. It was in reference probably to this com tion that some of the bitter sayings[1] of the schoolme others which have been recorded, were uttered.

Aristotle has given an intricate investigation of this ject, and his followers naturally were led along a si track. It would be quite foreign to my purpose in the sketch in this chapter to attempt to give any accou these enquiries, even were I competent to do so; for, a been pointed out, the connection between the Aristo modals and the modern view of the nature of Probal though real, is exceedingly slight. It need only be rem that what was complicated enough with four modals taken account of, grows intricate beyond all endurance such as the 'probable' and the 'true' and the 'false' also to be assigned a place in the list. The following ples[2] will show the kind of discussions with which the is exercised themselves. 'Whether, with one premise ain, and the other probable, a certain conclusion may be rred': 'Whether, from the impossible, the necessary can inferred'; 'Whether, with one premise necessary and the er *de inesse*, the conclusion is necessary', and so on, lessly.

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[1] "Haud scio magis ne doctrinam modalium scholastici exercuerint, quam ea illos vexarit. Certe usque adeo sudatum hic fuit, ut dicterio locus sit datus; *De modalibus non gustabit asinus.*" Keckermann, *Syst. Log.* Bk. II. ch. 3.

[2] *Smiglecii Disputationes,* stadt, 1618.

See also Prantl's *Geschic* *Logik* (under *Occam* and *B* for accounts of the excessiv plication which the subtlety o learned schoolmen evolved such suitable materials.

§ 22. On the Kantian view of modality the discussion uch kinds of syllogisms becomes at once decidedly more ple (for here but three modes are recognized), and also newhat more closely connected with strict Probability, (for modes are more nearly of the nature of gradations of viction). But, on the other hand, there is less justification their introduction, as logicians might really be expected know that what they are aiming to effect by their clumsy trivances is the very thing which Probability can carry to the highest desired degree of accuracy. The former thods are as coarse and inaccurate, compared with the ter, as were the roughest measurements of Babylonian ht-watchers compared with the refined calculations of the dern astronomer. It is indeed only some of the general erents of the Kantian Logic who enter upon any such siderations as these; some, such as Hamilton and Mansel, irely reject them, as we have seen. By those who do at of the subject, such conclusions as the following are laid wn; that when both premises are apodeictic the conclusion l be the same; so when both are assertory or problematic. one is apodeictic and the other assertory, the latter, or eaker,' is all that is to be admitted for the conclusion; l so on. The English reader will find some account of se rules in Ueberweg's *Logic*[1].

§ 23. But although those modals, regarded as instruments of accurate thought, have been thus superseded by the precise arithmetical expressions of Probability, the que still remains whether what may be termed our popular m expressions could not be improved and adapted to accurate use. It is true that the attempt to separate t from one another by any fundamental distinctions is fu for the magnitude of which they take cognizance is, as have remarked, continuous; but considering the enorm importance of accurate terminology, and of recogni numerical distinctions wherever possible, it would be a advance if any agreement could be arrived at with regar the use of modal expressions. We have already noticed II. § 16) some suggestions by Mr Galton as to the possibi of a natural system of classification, resting upon the re larity with which most kinds of magnitudes tend to gr themselves about a mean. It might be proposed, for stance, that we should agree to apply the term 'g to the first quarter, measuring from the best downwa 'indifferent' to the middle half, and 'bad' to the last qua There seems no reason why a similarly improved terminol should not some day be introduced into the ordinary m language of common life. It might be agreed, for insta that 'very improbable' should as far as possible be confi to those events which had odds of (say) more than 99 against them; and so on, with other similar expressi There would, no doubt, be difficulties in the way, for in applications of classification we have to surmount the t fold obstacles which lie in the way, firstly (to use Ka expression) of the faculty of making rules, and secondl that of subsumption under rules. That is to say, even if had agreed upon our classes, there would still be much do and dispute, in the case of things which did not readily l themselves to be counted or measured, as to whether odds were more or less than the assigned quantity.

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[1] Translation by T. M. Lindsay, p. 439.

[t is true that when we know the odds for or against an
ıt, we can always state them explicitly without the neces-
of first agreeing as to the usage of terms which shall
ly them. But there would often be circumlocution and
antry in so doing, and as long as modal terms are in
tical use it would seem that there could be no harm, and
ht be great good, in arriving at some agreement as to the
ree of probability which they should be generally under-
d to indicate. Bentham, as is well known, in despair of
r obtaining anything accurate out of the language of com-
ı life on this subject, was in favour of a direct appeal to
numerical standard. He proposed the employment, in
icial trials, of an instrument, graduated from 0 to 10, on
ch scale the witness was to be asked to indicate the de-
of his belief of the facts to which he testified: similarly
judge might express the force with which he held his
clusion. The use of such a numerical scale, however, was
be optional only, not compulsory, as Bentham admitted
t many persons might feel at a loss thus to measure the
ree of their belief. (*Rationale of Judicial Evidence*,
I., Ch. VI.)

§ 24. Throughout this chapter we have regarded the
dals as the nearest counterpart to modern Probability
ich was afforded by the old systems of logic. The reason
so regarding them is, that they represented some slight
mpt, rude as it was, to recognize and measure certain
dations in the degree of our conviction, and to examine
bearing of such considerations upon our logical inferences.
But although it is amongst the modals that the germs of
methods of Probability are thus to be sought; the true
ject-matter of our science, that is, the classes of objects
h which it is most appropriately concerned, are rather
resented by another part of the scholastic logic. This

was the branch commonly called Dialectic, in the ol of that term. Dialectic, according to Aristotle, se have been a sort of sister art to Rhetoric. It was con with syllogisms differing in no way from demonstrati logisms, except that their premises were probable ins certain. Premises of this kind he termed topics, a syllogisms which dealt with them enthymemes. The said to start from 'signs and likelihoods' rather tha axioms[1].

§ 25. The terms in which such reasonings are monly described sound very much like those applica Probability, as we now understand it. When we h likelihood, and of probable syllogisms, our first imp might be that the inferences involved would be of a character[2]. This, however, would be erroneous. I

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[1] "The εἰκὸς and σημεῖον themselves are propositions; the former stating a *general probability*, the latter a fact, which is known to be an indication, more or less certain, of the truth of some further statement, whether of a single fact, or of a general belief. The former is a general proposition, nearly, though not quite, universal; as, 'most men who envy hate'; the latter is a *singular* proposition, which however is not regarded as a sign, except relatively to some other proposition, which it is supposed may be inferred from it." (Mansel's Aldrich; Appendix F, where an account will be found of the Aristotelian enthymeme, and dialectic syllogism. Also, of course, in Grote's Aristotle, *Topics*, and elsewhere.)

[2] "Nam in hoc etiam differt demonstratio, seu demonstrati mentatio, à probabili, quia tam conclusio quam præmiss sariæ sunt; in probabili aute mentatione sicut conclusio ut bilis infertur ita præmissæ babiles afferuntur" (Crackan Bk. v., Ch. 1); almost the wor which De Morgan distinguis tween logic and probabilit passage already cited (see Ch.

Perhaps it was a develop some such view as this that L looked forward to. "J'ai dit pl fois qu'il faudrait une nouvell de Logique, qui traiteroit des de Probabilité, puisqu'Aristo ses Topiques n'a rien moins cela" (*Nouveaux essais*, Lib. xvi). It is possible, indeed, may have had in his mind mo we now understand by the m

ce the province of this Dialectic was much too wide,
vered in addition the whole field of what we should
m Scientific or Material Induction. The distinctive
ristic of the dialectic premises was their want of
y, and of such uncertain premises Probability (as I
equently insisted) takes account of one class only,
n concerning itself with another class. Again, not the
t attempt was made to enter upon the enquiry, *How*
n are the premises? It is only when this is at-
that we can be considered to enter upon the field of
lity, and it is because, after a rude fashion, the modals
ed to grapple with this problem, that we have regarded
in any way occupied with our special subject-matter.

. Amongst the older logics with which I have made
aintance, that of Crackanthorpe gives the fullest dis-
upon this subject. He divides his treatment of the
n into two parts, occupied respectively with the 'de-
tive' and the 'probable' syllogism. To the latter a
ook is devoted. In this the nature and consequences
en different 'loci'[1] are investigated, though it is not
ar in what sense they can every one of them be re-
s being 'probable.'

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y of Probability, but in the
f a science it is of course
ay whether any particular
definitely contemplated or
bnitz (as Todhunter has
his history) took the great-
t in such chance problems
been discussed.

ci were understood certain
asses of premises. They
fact, to the major premise
at the same relation that
ry or Predicament did to
the term. Crackanthorpe says of them, "sed duci a *loco probabiliter arguendi*, hoc vere proprium est Argumentationis probabilis; et in hoc a Demonstratione differt, quia Demonstrator utitur solummodo quatuor Locis eisque necessariis.... Præter hos autem, ex quibus quoque probabiliter arguere licet, sunt multo plures Loci arguendi probabiliter; ut a Genere, a Specie, ab Adjuncto, ab Oppositis, et similia" (*Logica*, Lib. v., ch. II.).

It is doubtless true, that if the old logicians had in possession of such premises as modern Probability is cerned with, and had adhered to their own way of tre them, they would have had to place them amongst such and thus to make the consideration of them a part of Dialectic. But inasmuch as there does not seem to been the slightest attempt on their part to do more than recognize the *fact* of the premises being probable; is, since it was not attempted to *measure* their probal and that of the conclusion, I cannot but regard this pa Logic as having only the very slightest relation to P bility as now conceived. It seems to me little more one of the ways (described at the commencement of chapter) by which the problem of Modality is not indee jected, but practically evaded.

§ 27. As Logic is not the only science which is di and prominently occupied with questions about belie evidence, so the difficulties which have arisen there been by no means unknown elsewhere. In respect o modals, this seems to have been manifestly the case in J prudence. Some remarks, therefore, may be conveni made here upon this application of the subject, thou course with the brevity suitable on the part of a layman has to touch upon professional topics.

Recall for a moment what are the essentials of mod These I understand to be the attempt to mark off fro another, without any resort to numerical notation, va degrees of conviction or belief, and to determine the c quent effect of premises, thus affected, upon our conclu Moreover, as we cannot construct or retain a scale o kind without employing a standard from and by whi measure it, the attainment and recognition of a standa certainty, or of one of the other degrees of convicti nost inseparably involved in the same enquiry. In this ase of the term, modal difficulties have certainly shown emselves in the department of Law. There have been nilar attempts here, encountered by similar difficulties, to me to some definite agreement as to a scale of arrange-ent of the degrees of our assent. It is of course much ore practicable to secure such agreement in the case of a ecial science, confined more or less to the experts, than in bjects into which all classes of outsiders have almost equal ght of entry. The range of application under the former rcumstances is narrower, and the professional experts have quired habits and traditions by which the standards may retained in considerable integrity. It does not appear, wever, according to all accounts, as if any very striking ccess had been attained in this direction by the lawyers.

§ 28. The difficulty in its scientific, or strictly jurispru-ntial shape, seems to have shown itself principally in the tempt to arrange legal evidence into classes in respect of e degree of its cogency. This, I understand, was the case the Roman law, and in some of the continental systems of risprudence which took their rise from the Roman law. The direct evidence of so many witnesses was *plena pro-tio.* Then came *minus plena probatio*, then *semiplenâ ajor* and *semiplenâ minor;* and by adding together a rtain number of half-proofs—for instance, by the pro-ction of a tradesman's account-books, *plus* his supple-entary oath—full proof might be made out. It was on is principle that torture was employed to obtain a con-sion. The confession was evidence suppletory to the cir-nstances which were held to justify its employment[1]."

According to Bentham[2], the corresponding scale in the

Stephen's *General View of the inal Law of England*, p. 241.

[2] *Rationale of Judicial Evidence;* Bk. I. ch. VI.

English school was:—Positive proof, Violent presu Probable presumption, Light or Rash presumption. admitted by Blackstone and others, I understand th divisions are not at all generally accepted at the day.

§ 29. In the above we are reminded rather o syllogisms. The principal practical form in which t culty underlying the simple modal propositions itself, is in the attempt to obtain some criterion of certainty. By 'certainty' here we mean, of course, n the metaphysicians term apodeictic[1], for that can se never be secured in practical affairs, but such a de conviction, short of this, as every reasonable person to be sufficient for all his wants. Here again, one think, the quest must appear, to accurate thinkers, an hopeless one; an effort to discover natural breaks i tinuous magnitude. There cannot indeed be the leas that, amongst limited classes of keen and practised in a standard of certainty, as of everything else, might tained and handed down with considerable accuracy: possible in matters of taste and opinion where persona liarities of judgment are far more liable to cause disagr and confusion. But then such a consensus is almost an affair of tact and custom; whereas what is wanted case in question is some criterion to which the compa uninitiated may be able to appeal. The standard, th must not merely be retained by recollection, but be ge recognizable by its characteristics. If such a criterio red, its importance could hardly be overrated. But s one may judge from the speeches of counsel, the of judges, and the verdicts of juries, nothing really ıg the name is ever attained.

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[1] Though this is claimed by some Kantian logicians;—Nie darf an einem angeblichen Verbrecher die gesetzliche Strafe vollzogen werden, *bevor* er nicht selbst das Verbrechen eingestanden. Denn wenn Zeugnisse und die übrigen wider ihn wären, so bleibt Gegentheil immer möglich' *Denklehre*, § 131).

). The nearest approach, perhaps, to a recognized d is to be found in the frequent assurance that juries bound to convict only in case they have *no* doubt of lt of the accused; for the absolute exclusion of all the utter impossibility of suggesting any counter sis which this assumes, is unattainable in human But, it is frequently said, they are to convict if ve no 'reasonable doubt,' no such doubt, that is, d be 'a hindrance to acting in the important affairs As a caution against seeking after unattainable y, such advice may be very useful; but it need be remarked that the certainty upon which we act in ortant affairs of life is no fixed standard, but varies ngly according to the nature of those affairs. The the reward at stake, the greater the risk we are d to run, and conversely. Hardly any degree of certan exist, upon the security of which we should not ared to act under appropriate circumstances[1].

ıe writers indeed altogether deny that any standard, common sense of the word, either is, or ought to be, at in legal proceedings. For instance, Sir J. F.

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[1] Ir C. J. Monro puts it: that a man is suspected of g his daughter. Evidence uld not convict him before ry jury might make a grand a true bill; evidence which t do this might make a jury bring in a verdict im; evidence which would not do this would very often prevent a Chancery judge from appointing the man guardian to a ward of the court; evidence which would not affect the judge's mind might make a father think twice on his death-bed before he appointed the man guardian to *his* daughter."

Stephen, in his work on English Criminal Law[1], noticing and rejecting such standards as that last cated, comes to the conclusion that the only standard nized by our law is that which induces juries to convi "What is judicial proof? That which being permitt law to be given in evidence, induces twelve men, c according to the Jury Act, to say that, having heard it, minds are satisfied of the truth of the proposition wh affirms. They may be prejudiced, they may be timid may be rash, they may be ignorant; but the oat number, and the property qualification, are intended, as possible, to neutralize these disadvantages, and precisely to the conditions imposed upon standards of or length." (p. 263.)

To admit this is much about the same thing as to ab such a standard as unattainable. Evidence which i a jury to convict may doubtless be a standard to m others of what we ought to consider 'reasonably c provided of course that the various juries are tolerab form in their conclusions. But it clearly cannot be pr as a standard to the juries themselves; if their decisi to be consistent and uniform, they want some externa cation to guide them. When a man is asking, *How* ought I to feel? to give such an answer as the ab surely, merely telling him that he is to be as cert is. If, indeed, juries composed a close profession, they ight, as was said above, retain a traditional standard. But ing, as they are, a selection from the ordinary lay public, eir own decisions in the past can hardly be held up to em as a direction what they are to do in future.

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[1] The portions of this work which treat of the nature of proof in general, and of judicial proof in particular, are well worth reading by every logical student. It appears to me, however, that the author goes much too far in the direction of regarding proof as subjective, that is as what *does* satisfy people, rather than as what *should* satisfy them. pares the legislative standar tainty with that of value; th is declared to be a certain w gold, irrespective of the commonness of that metal. certainty; if people grow mo lous the intrinsic value of th ard will vary.

§ 31. It would appear therefore that we may fairly say at the English law, at any rate, definitely rejects the main sumption upon which the logical doctrine of modality and s legal counterpart are based: the assumption, namely, that ifferent grades of conviction can be marked off from one other with sufficient accuracy for us to be able to refer dividual cases to their corresponding classes. And that ith regard to the collateral question of fixing a standard of rtainty, it will go no further than pronouncing, or im- ying, that we are to be content with nothing short of, but ed not go beyond, 'reasonable certainty.'

This is a statement of the standard, with which the gician and scientific man can easily quarrel; and they ay with much reason maintain that it has not the slightest aim to accuracy, even if it had one to strict intelligibility. a man wishes to know whether his present degree of cer- inty *is* reasonable, whither is he to appeal? He can arcely compare his mental state with that which is ex- rienced in 'the important affairs of life,' for these, as ready remarked, would indicate no fixed value. At the me time, one cannot suppose that such an expression is stitute of all signification. People would not continue to e language, especially in matters of paramount importance d interest, without meaning something by it. We are iven therefore to conclude that 'reasonable certainty' does a rude sort of way represent a traditional standard to ich it is attempted to adhere. As already remarked, this perfectly practicable in the case of any class of professional men, and therefore not altogether impossible in the c those who are often and closely brought into connection such a class. Though it is hard to believe that any expressions, when used for purposes of ordinary life, a at all near enough to any conventional standard to be discussion; yet in the special case of a jury, acting the direct influence of a judge, it seems quite possible their deliberate assertion that they are 'fully convi may reach somewhat more nearly to a tolerably fixed sta than ordinary outsiders would at first think likely.

§ 32. Are there then any means by which we ascertain what this standard is; in other words, by whi could determine what is the real worth, in respect of racy, of this 'reasonable certainty' which the juries are posed to secure? In the absence of authoritative de tions upon the subject, the student of Logic and Proba would naturally resort to two means, with a mome notice of which we will conclude this enquiry.

The first of these would aim at determining the sta of judicial certainty indirectly, by simply determining statistical frequency with which the decisions (say) of a were found to be correct. This may seem to be a ho task; and so indeed it is, but not so much on any the insufficiency of the determining elements as on accou the numerous arbitrary assumptions which attach to of the problems which deal with the probability of testi and judgments. It is not necessary for this purpose th should have an infallible superior court which revised decisions of the one under consideration[1]; it is sufficient

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[1] The question will be more fully discussed in a future chapter, but a few words may be inserted here by way of indication. Reduce the case to the simplest possible eleme supposing only two judges or o of the same average correctn decision. Let this be indicat

number of ordinary representative cases are submitted
ourt consisting even of exactly similar materials to the
hose decisions we wish to test. Provided always that we
the monstrous assumption that the judgments of men
t matters which deeply affect them are 'independent'
e sense in which the tosses of pence are independent,
the statistics of mere agreement and disagreement will
our purpose. We might be able to say, for instance,
a jury of a given number, deciding by a given majority,
right nine times out of ten in their verdict. Conclu-
of this kind, in reference to the French courts, are
Poisson has attempted at the end of his great work on
robability of Judgments; though I do not suppose that
tached much numerical accuracy to his results.

scarcely more hopeful means would be found by a
ence to certain cases of legal 'presumptions.' A 'con-
ve presumption' is defined as follows:—"Conclusive, or
ey are elsewhere termed imperative or absolute pre-
tions of law, are rules determining the quantity of evi-
e requisite for the support of any particular averment
h is not permitted to be overcome by any proof that the
is otherwise[1]." A large number of such presumptions
be found described in the text-books, but they seem to
to matters far too vague, for the most part, to admit of
reduction to statistical frequency of occurrence. It is
d maintained by some authorities that any assignment
gree of Probability is not their present object, but that
are simply meant to exclude the troublesome delays

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hen the chance of their agree-
$x^2+(1-x)^2$, for they agree if
are right or both wrong. If
tistical frequency of this agree-
is known,—that is, the fre-
with which the first judgment is confirmed by the second, we have the means of determining x.

[1] Taylor on Evidence: the latter part of the extract does not seem very clear.

that would ensue if everything were considered open doubt and question. Moreover, even if they did assign degree of certainty this would rather be an indication what legislators or judges thought reasonable than of wh was so considered by the juries themselves.

There are indeed presumptions as to the time after whi a man, if not heard of, is supposed to be dead (capable disproof, of course, by his reappearance). If this time varie with the age of the man in question, we should at once hav some such standard as we desire, for a reference to the Li tables would fix his probable duration of life, and so determine indirectly the measure of probability which satisfie the law. But this is not the case; the period chosen i entirely irrespective of age. The nearest case in point (an that does not amount to much) which I have been able t ascertain is that of the age after which it has been presumed that a woman was incapable of bearing childre This was the age of 53. A certain approach to a statistica assignment of the chances in this case is to be found in Quetelet's *Physique Sociale* (Vol. I. p. 184, note). Accordin to the authorities which he there quotes it would seem tha in about one birth in 5500 the mother was of the age of 50 or upwards. This does not quite assign the degree of wha may be called the à priori chance against the occurrence of birth at that age, because the fact of having commenced family at an early age represents some diminution of th probability of continuing it into later life. But it serves t give some indication of what may be called the odds agains such an event.

It need not be remarked that any such clues as these t the measure of judicial certainty are far too slight to be any real value. They only deserve passing notice as a possible logical solution of the problem in question, or rather dication of the mode in which, in theory, such a solution d have to be sought, were the English law, on these cts, a perfectly consistent scheme of scientific evidence. is the mode in which one would, under those circumes, attempt to extract from its proceedings an admission e exact measure of that standard of certainty which it ed, but which it declined openly to enunciate.

CHAPTER XIV.

FALLACIES.

§ 1. IN works on Logic a chapter is generally dev to the discussion of Fallacies, that is, to the descrip and classification of the different ways in which the r of Logic may be transgressed. The analogy of Probabi to Logic is sufficiently close to make it advisable to ac the same plan here. In describing his own opinions author is, of course, perpetually obliged to describe criticise those of others which he considers erroneous. some of the most widely spread errors find no suppor worth mentioning, and exist only in vague popular mis prehension. It will be found the best arrangement, th fore, at the risk of occasional repetition, to collect a few the errors that occur most frequently, and as far as possi to trace them to their sources; but it will hardly be wo the trouble to attempt any regular system of arrangem and classification. We shall mainly confine ourselves, accordance with the special province of this work, to proble which involve questions of logical interest, or to th which refer to the application of Probability to moral social science. We shall avoid the discussion of isola problems in games of chance and skill except when so error of principle seems to be involved in them.

§ 2. (I.) One of the most fertile sources of error and nfusion upon the subject has been already several times luded to, and in part discussed in a previous chapter. his consists in choosing the class to which to refer an vent, and therefore judging of the rarity of the event and e consequent improbability of foretelling it, *after it has appened*, and then transferring the impressions we expeience to a supposed contemplation of the event beforehand. he process in itself is perfectly legitimate (however unneessary it may be), since time does not in strictness enter t all into questions of Probability. No error therefore eed arise in this way, if we were careful as to the class hich we thus selected; but such carefulness is often negcted.

An illustration may afford help here. A man once ointed to a small target chalked upon a door, the target aving a bullet hole through the centre of it, and surprised ome spectators by declaring that he had fired that shot om an old fowling-piece at a distance of a hundred yards. is statement was true enough, but he suppressed a rather nportant fact. The shot had really been aimed in a general ay at the barn-door, and had hit it; the target was afterards chalked round the spot where the bullet struck. A eception analogous to this is, I think, often practised unconiously in other matters. We judge of events on a similar rinciple, feeling and expressing surprise in an equally uneasonable way, and deciding as to their occurrence on rounds which are really merely a subsequent adjunct of our wn. Butler's remarks about 'the story of Cæsar,' discussed lready in the twelfth chapter, are of this character. He lects a series of events from history, and then imagines a erson guessing them correctly who at the time had not the istory before him. As I have already pointed out, it is one thing to be unlikely to guess an event rightly wi specific evidence; it is another and very different thi appreciate the truth of a story which is founded parti entirely upon evidence. But it is a great mistake to tra to one of these ways of viewing the matter the mental pressions which properly belong to the other. It is drawing the target afterwards, and then being surpris find that the shot lies in the centre of it.

§ 3. One aspect of this fallacy has been already cussed, but it will serve to clear up difficulties which often felt upon the subject if we reexamine the que under a somewhat more general form.

In the class of examples under discussion we are gene presented with an individual which is not indeed defin referred to a class, but in regard to which we have no difficulty in choosing the appropriate class. Now sup we were contemplating such an event as the throwin sixes with a pair of dice four times running. Such a t would be termed a very unlikely event, as the odds ag its happening would be $36 \times 36 \times 36 \times 36 - 1$ to 1 or 167 to 1. The meaning of these phrases, as has been abund pointed out, is simply that the event in question occurs rarely; that, stated with numerical accuracy, it occurs on 1679616 times.

§ 4. But now let us make the assumption that throw has actually occurred; let us put ourselves into position of contemplating sixes four times running when known or reported that this throw has happened. The s phrase, namely that the event is a very unlikely one, often be used in relation to it, but we shall find that phrase may be employed to indicate, on one occasion another, extremely different meanings.

(1) There is, firstly, the most correct meaning. T

vent, it is true, has happened, and we know what it is, and herefore, we have not really any occasion to resort to the ules of Probability; but we can nevertheless conceive ourelves as being in the position of a person who does not now, and who has only Probability to appeal to. By calling he chances 1679615 to 1 against the throw we then mean o imply the fact, that inasmuch as such a throw occurs only nce in 1679616 times, our guess, were we to guess, would e correct only once in the same number of times; provided, hat is, that it is a fair guess, based simply on these statisical grounds.

§ 5. (2) But there is a second and very different coneption sometimes introduced, especially when the event in uestion is supposed to be known, not as above by the evilence of our experience, but by the report of a witness. We nay then mean by the 'chances against the event' (as was ointed out in Chapter XII.) not the proportional number of imes we should be right in guessing the event, but the roportional number of times the witness will be right in eporting it. The bases of our inference are here shifted n to new ground. In the former case the statistics were he throws and their respective frequency, now they are the witnesses' statements and their respective truthfulness.

§ 6. (3) But there is yet another meaning sometimes intended to be conveyed when persons talk of the chances against such an event as the throw in question. They may mean—not, Here is an event, how often should I have guessed it?—nor, Here is a report, how often will it be correct?—but something different from either, namely, Here is an event, how often will it be found to be produced by some one particular kind of cause?

When, for example, a man hears of dice giving the same throw several times running, and speaks of this as very

extraordinary, we shall often find that he is not
thinking of the improbability of his guess being rig
the report being true, but, that along with this, he i
ducing the question of the throw having been prod
fair dice. There is, of course, no reason whatever w
a question as this should not also be referred to Prob
provided always that we could find the appropriate st
by which to judge. These statistics would be compo
of throws of the particular dice, nor of reports of th
cular witness, but of the occasions on which such a t
the one in question respectively had, and had no
produced fairly. The objection to entering upon th
of the question would be that no such statistics are
able, and that if they were, we should prefer to fo
opinion (on principles to be described in Chapter XV
the special circumstances of the case rather than f
appeal to the average.

§ 7. The reader will easily be able to supply ex
in illustration of the distinctions just given; we will
examine but one. I hide a banknote in a certain bo
large library, and leave the room. A person tells m
after I went out, a stranger came in, walked straight
that particular book, and took it away with him.
people on hearing this account would reply, How ext
improbable! On analysing the phrase, I think w
find that certainly two, and possibly all three, of the
meanings might be involved in this exclamation. (1)
may be meant is this,—Assuming that the report i
and the stranger innocent, a rare event has occurred.
books might have been thus taken without that par
one being selected. I should not therefore have ex
the event, and when it has happened I am surprised.
a man has a perfect right to be surprised, but he

ical right (so long as we confine ourselves to this view) to
ke his surprise a ground for disbelieving the event. To
this is to fall into the fallacy described at the commence-
nt of this chapter. The fact of my not having been likely
have guessed a thing beforehand is no reason in itself for
ıbting it when I am informed of it. (2) Or I may stop
rt of the events reported, and apply the rules of Proba-
ity to the report itself. If so, what I mean is that such a
ry as this now before me is of a kind very generally false,
l that I cannot therefore attach much credit to it now.
Or I may accept the truth of the report, but doubt the
t of the stranger having taken the book at random. If
what I mean is, that of men who take books in the way
scribed, only a small proportion will be found to have
ken them really at random; the majority will do so because
y had by some means ascertained, or come to suspect,
at there was inside the book.

Each of the above three meanings is a possible and a
gitimate meaning. The only requisite is that we should be
reful to ascertain which of them is present to the mind, so
to select the appropriate statistics. The first makes in
elf the most legitimate use of Probability; the drawback
ing that at the time in question the functions of Pro-
bility are superseded by the event being otherwise known.
e second or third, therefore, is the more likely meaning to
present to the mind, for in these cases Probability, if it
uld be practically made use of, would, at the time in
estion, be a means of drawing really important inferences.
e drawbacks are the difficulty of finding such statistics,
d the extreme disturbing influence upon these statistics of
e circumstances of the special case.

§ 8. (II.) Closely connected with the tendency just
entioned is that which prompts us to confound a true

chance selection with one which is more or less p
When we are dealing with familiar objects in a co
way, especially when the greater rarity corresponds t
periority of quality, almost every one has learnt to rec
the distinction. No one, for instance, on observing
body of troops in a foreign town, but would be prompt
ask whether they came from an average regiment or
one that was picked. When however the distinction
to unfamiliar objects, and especially when only compa
rarity seems to be involved, the fallacy may assume a
subtle and misleading form, and seems to deserve s
notice by the consideration of a few examples.

Sometimes the result is not so much an actual fall
a slight misreckoning of the order of probability of the
under consideration. For instance, in the Pyramid que
we saw that it made some difference whether we consi
that π alone was to be taken into account or wheth
put this constant into a class with a small number of
similar ones. In deciding, however, whether or not th
anything remarkable in the actual falling short o
representation of the number 7 in the evaluation
(v. p. 248) the whole question turns upon consideratio
this kind. The only enquiry raised is whether there is
thing remarkable in this departure from the mean, an
answer depends upon whether we suppose that we a
ferring to a predetermined digit, or to whatever digit o
ten happens to be most above or below the average.
take the case raised by Cournot (*Exposition de la T*
des Chances, §§ 102, 114), that a certain deviation fro
mean in the case of Departmental returns of the prop
between male and female births is significant and indi
of a difference in kind, provided that we select at ran
single French Department; but that the same deviatio

e accidental if it is the maximum of the respective returns r several Departments[1]. The answer may be given one ay or the other according as we bear this consideration in ind.

§ 9. We are peculiarly liable to be misled in this way hen we are endeavouring to determine the cause of some henomenon, by mere statistics, in entire ignorance as to the irection in which the cause should be expected. In such ses an ingenious person who chooses to look about over a rge field can never fail to hit upon an explanation which is lausible in the sense that it fits in with the hitherto observed facts. With a tithe of the trouble which Mr Piazzi myth expended upon the measurement of the great pyramid, think I would undertake to find plausible intimations of veral of the important constants and standards which he scovered there, in the dimensions of the desk at which I am riting. The oddest instance of this sort of conclusion is erhaps to be found in the researches of a writer who has scovered[2] that there is a connection of a striking kind etween the respective successes of the Oxford and the mbridge boat in the annual race, and the greater and less equency of sun-spots.

Of course our usual practical resource in such cases is to ke appeal to our previous knowledge of the subject in estion, which enables us to reject as absurd a great number f hypotheses which can nevertheless make a fair show when ey are allowed to rest upon a limited amount of adroitly lected instances. But it must be remembered that if any eory chooses to appeal to statistics, to statistics it must be ffered to go for judgment. Even the boat race theory

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[1] Discussed by Mr F. Y. Edgeworth, the *Phil. Mag.* for April, 1887.

[2] *Journal of the Statistical Soc.* (Vol. XLII. p. 328) Dare one suspect a joke?

could be established (if sound) on this ground alone. T
is, if it really could be shown that experience in the long
confirmed the preponderance of successes on one side or
other according to the relative frequency of the sun-s
we should have to accept the fact that the two classe
events were not really independent. One of the two, wh
ever it may be, must be suspected of causing or influen
the other; or both must be caused or influenced by s
common circumstances.

II § 10. (III.) The fallacy described at the commencen
of this chapter arose from determining to judge of an
served or reported event by the rules of Probability,
employing a wrong set of statistics in the process of judg
Another fallacy, closely connected with this, arises from
practice of taking some only of the characteristics of suc
event, and arbitrarily confining to these the appeal to
bability. Suppose I toss up twelve pence, and find that el
of them give heads. Many persons on witnessing such an oc
rence would experience a feeling which they would expres
the remark, How near that was to getting all heads! A
any thing very important were staked on the throw
would be much excited at the occurrence. But in
sense were we near to twelve? There is a not uncom
error, I apprehend, which consists in unconsciously regar
the eleven heads as a thing which is already some
secured, so that one might as it were keep them, and
take our chance for securing the remaining one. The el
are mentally set aside, looked upon as certain (for they
already happened), and we then introduce the notio
chance merely for the twelfth. But this twelfth, ha
also happened, has no better claim to such a distinction
any of the others. If we will introduce the notion of ch
in the case of the one that gave tail we must do the sam

he case of all the others as well. In other words, if the osser be dissatisfied at the appearance of the one tail, and ish to cancel it and try his luck again, he must toss up the hole lot of pence again fairly together. In this case, of ourse, so far from his having a better prospect for the next hrow he may think himself in very good luck if he makes gain as good a throw as the one he rejected. What he is oing is confounding this case with that in which the throws re really *successive*. If eleven heads have been tossed up in urn, we are of course within an even chance of getting a welfth; but the circumstances are quite different in the stance proposed.

§ 11. In the above example the error is transparent. ut in forming a judgment upon matters of greater comlexity than dice and pence, especially in the case of what are lled 'narrow escapes,' a mistake of an analogous kind is, I pprehend, far from uncommon. A person, for example, who s just experienced a narrow escape will often be filled ith surprise and anxiety amounting almost to terror. The ent being past, these feelings are, at the time, in strictness appropriate. If, as is quite possible, they are merely inctive, or the result of association, they do not fall within e province of any kind of Logic. If, however, as seems ore likely, they partially arise from a supposed transference f ourselves into that point of past time at which the event as just about to happen, and the production by imaginaon of the feelings we should then expect to experience, his process partakes of the nature of an inference, and can e right or wrong. In other words, the alarm may be prortionate or disproportionate to the amount of danger that ight fairly have been reckoned upon in such a hypothetical nticipation. If the supposed transfer were completely rried out, there would be no fallacy; but it is often very incompletely done, some of the component parts of the ev being supposed to be determined or 'arranged' (to us sporting phrase) in the form in which we now know t they actually have happened, and only the remaining c being fairly contemplated as future chances.

A man, for example, is out with a friend, whose rifle g off by accident, and the bullet passes through his hat. trembles with anxiety at thinking what might have h pened, and perhaps remarks, 'How very near I was to be killed!' Now we may safely assume that he means so thing more than that a shot passed very close to him. has some vague idea that, as he would probably say, ' chance of being killed then was very great.' His surp and terror may be in great part physical and instinct arising simply from the knowledge that the shot had pas very near him. But his mental state may be analysed, a we shall then most likely find, at bottom, a fallacy of kind described above. To speak or think of chance in c nection with the incident, is to refer the particular incid to a class of incidents of a similar character, and then to c sider the comparative frequency with which the cont plated result ensues. Now the series which we may supp to be most naturally selected in this case is one compose shooting excursions with his friend; up to this point proceedings are assumed to be designed, beyond it o in the subsequent event, was there accident. Once i thousand times perhaps on such occasions the gun will off accidentally; one in a thousand only of those dischar will be directed near his friend's head. If we will make accident a matter of Probability, we ought by rights in t way (to adopt the language of the first example), to 'toss again' fairly. But we do not do this; we seem to assume *certain* that the shot goes within an inch of our heads, ch that from the notion of chance at all, and then begin to troduce this notion again for possible deflections from that ving inch.

§ 12. (IV.) We will now notice a fallacy connected with he subjects of betting and gambling. Many or most of the opular misapprehensions on this subject imply such utter norance and confusion as to the foundations of the science at it would be needless to discuss them here. The follow-g however is of a far more plausible kind, and has been a urce of perplexity to persons of considerable acuteness.

The case, put into the simplest form, is as follows[1]. ppose that a person *A* is playing against *B*, *B* being ther another individual or a group of individuals, say a mbling bank. They begin by tossing for a shilling, and maintains that he is in possession of a device which will sure his winning. If he does win on the first occasion he s clearly gained his point so far. If he loses, he stakes ext time two shillings instead of one. The result of course that if he wins on the second occasion he replaces his rmer loss, and is left with one shilling profit as well. So e goes on, doubling his stake after every loss, with the bvious result that on the first occasion of success he makes od all his previous losses, and is left with a shilling over. t such an occasion must come sooner or later, by the sumptions of chance on which the game is founded. Hence follows that he can insure, sooner or later, being left a al winner. Moreover he may win to any amount; firstly om the obvious consideration that he might make his itial stake as large as he pleased, a hundred pounds, for instance, instead of a shilling; and secondly, because he has done once he may do again. He may put his sh by, and have a second spell of play, long or short as the may be, with the same termination to it. According mere persistency he may accumulate any sum of mon pleases, in apparent defiance of all that is meant by lucl

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[1] It appears to have been long own to gamblers under the name the *Martingale*. There is a paper Babbage (*Trans. of Royal Soc. of Edinburgh*, for 1823) which discusses certain points connected with it, but scarcely touches on the subject of the sections which follow.

§ 13. I have classed this opinion among fallacies, a present is the most convenient opportunity of discussi though in strictness it should rather be termed a par since the conclusion is perfectly sound. The only fa consists in regarding such a way of obtaining the resı mysterious. On the contrary, there is nothing more than to insure ultimate success under the given condi The point is worth enquiry, from the principles it inv and because the answers commonly given do not quite the difficulty. It is sometimes urged, for instance, th bank would or does allow the speculator to choose at wil amount of his stake, but puts a limit to the amoun which it will consent to play. This is quite true, but course no answer to the hypothetical enquiry before us, w assumes that such a state of things *is* allowed. Again, i been urged that the possibility in question turns ent upon the fact that credit must be supposed to be give otherwise the fortune of the player may not hold out his turn of luck arrives:—that, in fact, sooner or later, i goes on long enough, his fortune will not hold out enough, and all his gains will be swept away. It is c true that credit is a *condition* of success, but it is in no s the cause. We may suppose both parties to agree at outset that there shall be no payments until the gam ended, *A* having the right to decide when it shall be sidered to be ended. It still remains true that where *ordinary* gambling, i.e. with fixed or haphazard stake could not ensure winning eventually to any extent, he can do so if he adopt such a scheme as the one in question. And this is the state of things which seems to call for explanation.

§ 14. What causes perplexity here is the supposed fact that in some mysterious way certainty has been conjured out of uncertainty; that in a game where the detailed events are utterly inscrutable, and where the average, by supposition, shows no preference for either side, one party is nevertheless succeeding somehow in steadily drawing the luck his own way. It looks as if it were a parallel case with that of a man who should succeed by some device in permanently securing more than half of the tosses with a penny which was nevertheless to be regarded as a perfectly fair one.

This is quite a mistake. The real fact is that *A* does not expose his gains to chance at all; all that he so exposes is the number of times he has to wait until he gains. Put such a case as this. I offer to give a man any sum of money he chooses to mention provided he will at once give it back again to me with one pound more. It does not need much acuteness to see that it is a matter of indifference to me whether he chooses to mention one pound, or ten, or a hundred. Now suppose that instead of leaving it to his choice which of these sums is to be selected each time, the two parties agree to leave it to chance. Let them, for instance, draw a number out of a bag each time, and let that be the sum which *A* gives to *B* under the prescribed conditions. The case is not altered. *A* still gains his pound each time, for the introduction of the element of chance has not in any way touched this. All that it does is to make this pound the result of an uncertain subtraction, sometimes 10 *minus* 9, sometimes 50 *minus* 49, and so on. It is these numbers only, not their difference, which he submits to luck, and this is of no consequence whatever.

To suggest to any individual or company that should consent to go on playing upon such terms as would be too barefaced a proposal. And yet the ca question is identical in principle, and almost identic form, with this. To offer to give a man any sum he lik name provided he gives you back again that same sum one, and to offer him any number of terms he pleases o series 1, 2, 4, 8, 16, &c., provided you have the next ter the set, are equivalent. The only difference is that ir latter case the result is attained with somewhat mo arithmetical parade. Similarly equivalent are the proc in case we prefer to leave it to chance, instead of to cl to decide what sum or what number of terms shall be upon. This latter is what is really done in the ca question. A man who consents to go on doubling his every time he wins, is leaving nothing else to chance the determination of the particular number of terms of a geometrical series which shall be allowed to pass befo stops.

§ 15. It may be added that there is no special virt the particular series in question, viz. that in accordance which the stake is doubled each time. All that is nee that the last term of the series should more than balan the preceding ones. Any other series which increased than this geometrical one, would answer the purpose a or better. Nor is it necessary, again, that the game s be an even or ‘fair’ one. Chance, be it remembered, a nothing here but the number of terms to which the attains on each occasion, its final result being always metically fixed. When a penny is tossed up it is on one of every two occasions that the series runs to more two terms, and so his fixed gains come in pretty regu *But* unless he was playing for a limited time only, it ot affect him if the series ran to two hundred terms; it rould merely take him somewhat longer to win his stakes. A man might safely, for instance, continue to lay an *even* bet that he would get the single prize in a lottery of a thousand tickets, provided he thus doubled, or more than doubled, his stake each time, and unlimited credit was given.

§ 16. So regarded, the problem is simple enough, but there are two points in it to which attention may conveniently be directed.

In the first place, it serves very pointedly to remind us of the distinction between a series of events (in this case the osses of the penny) which really are subjects of chance, and ur conduct founded upon these events, which may or may ot be so subject[1]. It is quite possible that this latter may e so contrived as to be in many respects a matter of absolute certainty,—a consideration, I presume, familiar enough o professional betting men. Why is the ordinary way of etting on the throws of a penny fair to both parties? Because a 'fair' series is 'fairly' treated. The heads and tails ccur at random, but on an average equally often, and the takes are either fixed or also arranged at random. If a man acks heads every time for the same amount, he will of ourse in the long run neither win nor lose. Neither will he he varies the stake every time, provided he does not vary in such a way as to make its amount dependent on the ct of his having won or lost the time before. But he ay, if he pleases, and the other party consents, so arrange is stakes (as in the case in question) that Chance, if one ight so express it, does not get a fair chance. Here the uman elements of choice and design have been so brought bear upon a series of events which, regarded by themselves, exhibit nothing but the physical characteristi chance, that the latter elements disappear, and we g result which is arithmetically certain. Other analo instances might be suggested, but the one before us has merit of most ingeniously disguising the actual process.

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[1] Attention will be further directed to this distinction in the chapter on surance and Gambling.

§ 17. The meaning of the remark just made wil better seen by a comparison with the following case. It been attempted[1] to explain the preponderance of male b over female by assuming that the chances of the two equal, but that the general desire to have a male heir t to induce many unions to persist until the occurrence of event, and no longer. It is supposed that in this way t would be a slight preponderance of families which consi of one son only, or of two sons and one daughter, and so f

This is quite fallacious (as had been noticed by Lap in his *Essai*); and there could not be a better inst chosen than this to show just what we can do and wha cannot do in the way of altering the luck in a real cha succession of events. To suppose that the number of a births could be influenced in the way in question is ex the same thing as to suppose that a number of gam could increase the ratio of heads to tails, to something one-half, by each handing the coin to his neighbour as as he had thrown a head: that they have only to leave soon as head has appeared; an absurdity which we need pause to explain at this stage. The essential point a the 'Martingale' is that, whereas the occurrence of events on which the stakes are laid is unaffected, the st themselves can be so adjusted as to make the luck swin way.

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[1] As by Prevost in the *Bibliothèque Universelle de Genève*, Oct. 1829. *The* explanation is noted, and apparently accepted, by Quetelet *sique Sociale* I. 171).

§ 18. In the second place, this example brings before us what has had to be so often mentioned already, namely, that the series of Probability are in strictness supposed to be interminable. If therefore we allow either party to call upon us to stop, especially at a point which just happens to suit him, we may get results decidedly opposed to the integrity of the theory. In the case before us it is a necessary stipulation for *A* that he may be allowed to leave off when he wishes, that is at one of the points at which the throw is in his favour. Without this stipulation he may be left a loser to any amount.

Introduce the supposition that one party may arbitrarily call for a stoppage when it suits him and refuse to permit it sooner, and almost any system of what would be otherwise fair play may be converted into a very one-sided arrangement. Indeed, in the case in question, *A* need not adopt this device of doubling the stakes every time he loses. He may play with a fixed stake, and nevertheless insure that *one* party shall win any assigned sum, assuming that the game is even and that he is permitted to play on credit.

§ 19. (V.) A common mistake is to assume that a very unlikely thing will not happen at all. It is a mistake which, when thus stated in words, is too obvious to be committed, for the meaning of an unlikely thing is one that happens at rare intervals; if it were not assumed that the event would happen sometimes it would not be called unlikely, but impossible. This is an error which could scarcely occur except in vague popular misapprehension, and is so abundantly refuted in works on Probability, that it need only be touched upon briefly here. It follows of course, from our definition of Probability, that to speak of a very rare combination of events as one that is 'sure never to happen,' is to use language incorrectly. Such a phrase may pass current as a loose popular exaggeration, but in strictness it involv contradiction. The truth about such rare events canno better described than in the following quotation from Morgan[1]:—

"It is said that no person ever *does* arrive at such tremely improbable cases as the one just cited [drawing same ball five times running out of a bag containing tw balls]. That a given individual should never throw an twelve times running on a single die, is by far the n likely; indeed, so remote are the chances of such an e in any twelve trials (more than 2,000,000,000 to 1 agains that it is unlikely the experience of any given country any given century, should furnish it. But let us stop f moment, and ask ourselves to what this argument app A person who rarely touches dice will hardly believe doublets sometimes occur three times running; one who dles them frequently knows that such is sometimes the Every very practised user of those implements has seen rarer sequences. Now suppose that a society of persons thrown the dice so often as to secure a run of six aces served and recorded, the preceding argument would stil used against twelve. And if another society had pract long enough to see twelve aces following each other, might still employ the same method of doubting as to a of twenty-four; and so on, *ad infinitum.* The power of i gining cases which contain long combinations so much ceeds that of exhibiting and arranging them, that it is to assign a telegraph which should make a separate si for every grain of sand in a globe as large as the visible verse, upon the hypothesis of the most space-penetra astronomer. The fallacy of the preceding objection lie supposing events in number beyond our experience, comp ntirely of sequences such as fall within our experience. It akes the past necessarily contain the whole, as to the quaity of its components; and judges by samples. Now the east cautious buyer of grain requires to examine a handful efore he judges of a bushel, and a bushel before he judges f a load. But relatively to such enormous numbers of cominations as are frequently proposed, our experience does not eserve the title of a handful as compared with a bushel, or ven of a single grain."

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[1] *Essay on Probabilities*, p. 126.

§ 20. The origin of this inveterate mistake is not diffiult to be accounted for. It arises, no doubt, from the exiencies of our practical life. No man can bear in mind very contingency to which he may be exposed. If therefore e are ever to do anything at all in the world, a large numer of the rarer contingencies must be left entirely out of ccount. And the necessity of this oblivion is strengthened y the shortness of our life. Mathematically speaking, it ould be said to be certain that any one who lives long nough will be bitten by a mad dog, for the event is not an mpossible, but only an improbable one, and must therefore ome to pass in time. But this and an indefinite number f other disagreeable contingencies have on most occasions o be entirely ignored in practice, and thence they come lmost necessarily to drop equally out of our thought and xpectation. And when the event is one in itself of no imortance, like a rare throw of the dice, a great effort of magination may be required, on the part of persons not acustomed to abstract mathematical calculation, to enable em to realize the throw as being even possible.

Attempts have sometimes been made to estimate what xtremity of unlikelihood ought to be considered as equialent to this practical zero point of belief. In so far as ch attempts are carried out by logicians, or by those who are unwilling to resort to mathematical valuation of cl they must be regarded as merely a special form of the difficulties discussed in the last chapter, and need not fore be reconsidered here; but a word or two may be concerning the views of some who have looked at the : from the mathematician's point of view.

The principal of these is perhaps Buffon. He l rived at the estimate (*Arithmétique Morale* § VIII.) th practical zero is equivalent to a chance of $\frac{1}{10,000}$. grounds for selecting this fraction are found in the fac according to the tables of mortality accessible to h represents the chance of a man of 56 dying in the cou the next day. But since no man under common ci stances takes the chance into the slightest considerat follows that it is practically estimated as having no val

It is obvious that this result is almost entirely arb and in fact his reasons cannot be regarded as anything than a slender justification from experience for adop conveniently simple fraction; a justification however would apparently have been equally available in the c any other fractions lying within wide limits of th selected[1].

§ 21. There is one particular form of this error, from the importance occasionally attached to it, de perhaps more special examination. As stated above, can be no doubt that, however unlikely an event may we (loosely speaking) vary the circumstances sufficien

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[1] This theoretical or absolute neglect of what is very rare must not be confused with the practical neglect sometimes recommended by astronomical and other observers. A criterion, known as Chauvenet's, for *indicating* the limits of such rejection will be found describe Merriman's *Least Squares* (1 But this rests on the unders that a smaller balance of erro thus result in the long ru very rare event is delibera jected, not overlooked.

, in other words, we keep on trying long enough, we shall meet with such an event at last. If we toss up a pair of ice a few times we shall get doublets; if we try longer with hree we shall get triplets, and so on. However unusual the vent may be, even were it sixes a thousand times running, t will come some time or other if we have only patience and itality enough. Now apply this result to the letters of the lphabet. Suppose that one letter at a time is drawn from bag which contains them all, and is then replaced. If the letters were written down one after another as they occurred, t would commonly be expected that they would be found to make mere nonsense, and would never arrange themselves nto the words of any language known to men. No more hey would in general, but it is a commonly accepted result f the theory, and one which we may assume the reader to e ready to admit without further discussion, that, if the rocess were continued long enough, words making sense ould appear; nay more, that any book we chose to menion,—Milton's *Paradise Lost* or the plays of Shakespeare, r example,—would be produced in this way at last. It ould take more days than we have space in this volume to epresent in figures, to make tolerably certain of obtaining he former of these works by thus drawing letters out of a g, but the desired result would be obtained at length[1].

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[1] The process of calculation may readily indicated. There are, say, out 350,000 letters in the work in estion. Since any of the 26 letters the alphabet may be drawn each e, the possible number of comations would be $26^{350,000}$; a numr which, as may easily be inferred m a table of logarithms, would mand for its expression nearly 0,000 figures. Only one of these combinations is favourable, if we reject variations of spelling. Hence unity divided by this number would represent the chance of getting the desired result by successive random selection of the required number of 350,000 letters.

If this chance is thought too small, and any one asks how often the above random selection must be repeated in order to give him odds of 2

Now many people have not unnaturally thought rogatory to genius to suggest that its productions have also been obtained by chance, whilst others have on to argue, If this be the case, might not the world it this manner have been produced by chance?

§ 22. We will begin with the comparatively simp terminate, and intelligible problem of the possible prod of the works of a great human genius by chance. regard to this possibility, it may be a consolation to timid minds to be reminded that the power of produci works of a Shakespeare, *in time*, is not confined to co mate genius and to mere chance. There is a third al tive, viz. that of purely mechanical procedure. An down almost to an idiot, might do it, if he took suf time about the task. For suppose that the required n of letters were procured and arranged, not by chanc designedly, and according to rules suggested by the of permutations: the letters of the alphabet and the n of them to be employed being finite, every order in they could occur would come in its due turn, and the every thing which can be expressed in language wo arrived at some time or other.

There is really nothing that need shock any one i a result. Its possibility arises from the following

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to 1 in favour of success, this also can be easily shown. If the chance of an event on each occasion is $\frac{1}{n}$, the chance of getting it once at least in n trials is $1-\left(\frac{n-1}{n}\right)^n$; for we shall do this unless we fail n times running. When (as in the case in *question*) n is very large, this may be shown algebraically to be eq to odds of about 2 to 1. T when we have drawn the r quantity of letters a number c equal to the inconceivably number above represented, it only 2 to 1 that we shall h cured what we want:—and t have to recognize it.

The number of letters, and therefore of words, at our disposal s limited; whatever therefore we may desire to express n language necessarily becomes subject to corresponding imitation. The possible variations of thought are literally nfinite, so are those of spoken language (by intonation of the voice, &c.); but when we come to words there is a limitation, the nature of which is distinctly conceivable by the mind, though the restriction is one that in practice will never be appreciable, owing to the fact that the number of combinations which may be produced is so enormous as to surpass all power of the imagination to realize[1]. The answer therefore is plain, and it is one that will apply to many other cases as well, that to put a finite limit upon the number of ways in which a thing can be done, is to determine that any one who is able and willing to try long enough shall succeed in doing it. If a great genius condescends to perform it under these circumstances, he must submit to the possibility of having his claims rivalled or disputed by the chance-man and idiot. If Shakespeare were limited to the use of eight or nine assigned words, the time within which the latter agents might claim equality with him would not be very great. As it is, having had the range of the English language at his disposal, his reputation is not in danger of being assailed by any such methods.

§ 23. The case of the possible production of the world by chance leads us into an altogether different region of discussion. We are not here dealing with figures the nature and use of which are within the fair powers of the understanding, however the imagination may break down in attempting to realize the smallest fraction of their full significance. The understanding itself is wandering out ‹ proper province, for the conditions of the problem cann assigned. When we draw letters out of a bag we know well what we are doing; but what is really meant by ducing a world by chance? By analogy of the former we may assume that some kind of agent is presuppos‹ perhaps therefore the following supposition is less al than any other. Imagine some being, not a Creator l sort of Demiurgus, who has had a quantity of material into his hands, and he assigns them their collocation‹ their laws of action, blindly and at haphazard: what ar odds that such a world as we actually experience should been brought about in this way?

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[1] The longest life which could reasonably be attributed to any language would of course dwindle into utter insignificance in the face of such periods of time as are being here arithmetically contemplated.

If it were worth while seriously to set about answ such a question, and if some one would furnish us witl number of the letters of such an alphabet, and the leng the work to be written with them, we could proceed to cate the result. But so much as this may surely be affi about it;—that, far from merely finding the length ol small volume insufficient for containing the figures in ‹ the adverse odds would be given, all the paper whicl world has hitherto produced would be used up before w‹ got far on our way in writing them down.

§ 24. The most seductive form in which the diffi about the occurrence of very rare events generally pre itself is probably this. 'You admit (some persons wi disposed to say) that such an event may sometimes ha‹ nay, that it does sometimes happen in the infinite cou‹ time. How then am I to know that *this* occasion is no of these possible occurrences?' To this, one answer onl be given,—the same which must always be given w statistics and probability are concerned,—'The present *be such* an occasion, but it is inconceivably unlikely th should be one. Amongst countless billions of times in which you, and such as you, urge this, one person only will be justified; and it is not likely that you are that one, or that this is that occasion.'

§ 25. There is another form of this practical inability to distinguish between one high number and another in the estimation of chances, which deserves passing notice from its importance in arguments about heredity. People will often urge an objection to the doctrine that qualities, mental and bodily, are transmitted from the parents to the offspring, on the ground that there are a multitude of instances to the contrary, in fact a great majority of such instances. To raise this objection implies an utter want of appreciation of the very great odds which possibly may exist, and which the argument in support of heredity implies *do* exist against any given person being distinguished for intellectual or other eminence. This is doubtless partly a matter of definition, depending upon the degree of rarity which we consider to be implied by eminence; but taking any reasonable sense of the term, we shall readily see that a very great proportion of failures may still leave an enormous preponderance of evidence in favour of the heredity doctrine. Take, for instance, that degree of eminence which is implied by being one of four thousand. This is a considerable distinction, though, since there are about two thousand such persons to be found amongst the total adult male population of Great Britain, it is far from implying any conspicuous genius. Now suppose that in examining the cases of a large number of the children of such persons, we had found that 199 out of 200 of them failed to reach the same distinction. Many persons would conclude that this was pretty conclusive evidence against any hereditary transmission. To be able to adduce only one favourable, as *against* 199 hostile instances, would to them represent the entire break-down of any such The error, of course, is obvious enough, and one whic the figures thus before him, hardly any one could avoid. But if one may judge from common conve and other such sources of information, it is found in p exceedingly difficult adequately to retain the convicti even though only one in 200 instances were favo this would represent odds of about 20 to 1 in favour theory. If hereditary transmission did not prevail, o in 4000 sons would thus rival their fathers; but w actually, let us say (we are of course taking imagina portions here), that one in 200 does. Hence, if the st are large enough to be satisfactory, there has been influence at work which has improved the chances o coincidence in the ratio of 20 to 1. We are in fact s able to realise the meaning of very large numbers,—t to retain the *ratios* in the mind, where large numb concerned,—that unless we repeatedly check oursel arithmetical considerations we are too apt to treat an mate all beyond certain limits as equally vast and vag

§ 26. (VI.) In discussing the nature of the con between Probability and Induction, we examined the of a rule commonly given for inferring the probabilit an event which had been repeatedly observed would again. I endeavoured to show that all attempts to and prove such a rule were necessarily futile; if these were conclusive the employment of such a rule m course be regarded as fallacious. A few examples ma veniently be added here, tending to show how inst there being merely a single rule of succession we might divide the possible forms into three classes.

(1) In some cases when a thing has been obser happen several times it becomes in consequence *more*

at the thing should happen again. This agrees with the dinary form of the rule, and is probably the case of most equent occurrence. The necessary vagueness of expression hen we talk of the 'happening of a thing' makes it quite npossible to tolerate the rule in this general form, but if we ecialize it a little we shall find it assume a more familiar ape. If, for example, we have observed two or more proerties to be frequently associated together in a succession of dividuals, we shall conclude with some force that they will e found to be so connected in future. The strength of our onviction however will depend not merely on the number f observed coincidences, but on far more complicated coniderations; for a discussion of which the reader must be eferred to regular treatises on Inductive evidence. Or again, we have observed one of two events succeed the other everal times, the occurrence of the former will excite in ost cases some degree of expectation of the latter. As efore, however, the degree of our expectation is not to be ssigned by any simple formula; it will depend in part upon he supposed intimacy with which the events are connected. o attempt to lay down definite rules upon the subject ould lead to a discussion upon laws of causation, and the rcumstances under which their existence may be inferred, nd therefore any further consideration of the matter must e abandoned here.

§ 27. (2) Or, secondly, the past recurrence may in itlf give no valid grounds for inference about the future; is is the case which most properly belongs to Probability[1].

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[1] We are here assuming of course at the ultimate limit to which our erage tends is known, either from owledge of the causes or from preous extensive experience. We are assuming that *e.g.* the die is known to be a fair one; if this is not known but a possible bias has to be inferred from its observed performances, the case falls under the former head.

That it does so belong will be easily seen if we bear in n the fundamental conception of the science. We are tl introduced to a series,—for purposes of inference an inc nitely extended series,—of terms, about the details of wh information, except on certain points, is not given; our kn ledge being confined to the statistical fact, that, say, on ten of them has some attribute which we will call X. S pose now that five of these terms in succession have been what hint does this give about the sixth being also an Clearly none at all; this past fact tells us nothing; the mula for our inference is still precisely what it was bef that one in ten being X it is one to nine that the next t is X. And however many terms in succession had been one kind, precisely the same formula would still be given.

§ 28. The way in which events will justify the ans given by this formula is often misunderstood. For benefit therefore of those unacquainted with some of conceptions familiar to mathematicians, a few words of planation may be added. Suppose then that we have X twelve times in succession. This is clearly an anomal state of things. To suppose anything like this continu to occur would be obviously in opposition to the statist which assert that in the long run only one in ten is But how is this anomaly got over? In other words, l do we obviate the conclusion that X's must occur m frequently than once in ten times, after such a long succ sion of them as we have now had? Many people seem believe that there must be a diminution of X's afterwa to counterbalance their past preponderance. This howe would be quite a mistake; the proportion in which t occur in future must remain the same throughout; it can be altered if we are to adhere to our statistical form *The fact* is that the rectification of the exceptional distu in the proportion will be brought about simply by the ıual influx of fresh terms in the series. These will in ong run neutralize the disturbance, not by any special ation, as it were, for the purpose, but by the mere ıt of their overwhelming numbers. At every stage fore, in the succession, whatever might have been the er and nature of the preceding terms, it will still be o say that one in ten of the terms will be an X.

we had to do only with a finite number of terms, ver large that number might be, such a disturbance as ve spoken of would, it is true, need a special alteration e subsequent proportions to neutralize its effects. But we have to do with an infinite number of terms, this the case; the 'limit' of the series, which is what we have to deal with, is unaffected by these temporary bances. In the continued progress of the series we find, as a matter of fact, more and more of such disnces, and these of a more and more exceptional character. hatever the point we may occupy at any time, if we orward or backward into the indefinite extension of the we shall still see that the ultimate limit to the pron in which its terms are arranged remains the same; t is with this limit, as above mentioned, that we are rned in the strict rules of Probability.

ıe most familiar example, perhaps, of this kind is that sing up a penny. Suppose we have had four heads in sion; people[1] have tolerably realized by now that 'head fth time' is still an even chance, as 'head' was each

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[1] ept indeed the gamblers. Acto a gambling acquaintance loudin, the conjurer, describes as having met at Spa, "the a particular combination has l the more certain it is that it will not be repeated at the next *coup:* this is the groundwork of all theories of probabilities and is termed the maturity of chances" (*Cardsharping exposed*, p. 85).

time before, and will be ever after. The preceding
graph explains how it is that these occasional disturba
in the average become neutralized in the long run.

§ 29. (3) There are other cases which, though
are by no means unknown, in which such an inferenc
that obtained from the Rule of Succession would be th
rect reverse of the truth. The oftener a thing happe
may be, the more unlikely it is to happen again. This i
case whenever we are drawing things from a limited s
(as balls from a bag without replacing them), or whe
the act of repetition itself tends to prevent the succe
(as in giving false alarms).

I am quite ready to admit that we believe the result
scribed in the last two classes on the strength of some
general Inductive rule, or rather principle, as that inv
in the first. But it would be a great error to confound
with an admission of the validity of the rule in each s
instance. We are speaking about the application of the
to individual cases, or classes of cases; this is quite a
tinct thing, as was pointed out in a previous chapter,
giving the grounds on which we rest the rule itself.
man were to lay it down as a universal rule, that the
mony of all persons was to be believed, and we adduc
instance of a man having lied, it would not be consi
that he saved his rule by showing that we believed th
was a lie on the word of other persons. But it is per
consistent to give as a merely general, but not univ
rule, that the testimony of men is credible; then to sep
off a second class of men whose word is not to be tr
and finally, if any one wants to know our ground fo
second rule, to rest it upon the first. If we were spe
of *necessary* laws, such a conflict as this would
hopeless as the old 'Cretan' puzzle in logic; but i

nces of Inductive and Analogical extension it is perfectly rmless.

§ 30. A familiar example will serve to bring out the ree different possible conclusions mentioned above. We ve observed it rain on ten successive days. *A* and *B* con-ide respectively for and against rain on the eleventh day; maintains that the past rain affords no data whatever for opinion. Which is right? We really cannot determine ? *priori.* An appeal must be made to direct observation, or ans must be found for deciding on independent grounds which class we are to refer the instance. If, for example, were known that every country produces its own rain, we uld choose the third rule, for it would be a case of drawing m a limited supply. If again we had reasons to believe t the rain for our country might be produced anywhere the globe, we should probably conclude that the past nfall threw no light whatever on the prospect of a con-uance of wet weather, and therefore take the second. if, finally, we knew that rain came in long spells or sea-s, as in the tropics, then the occurrence of ten wet days uccession would make us believe that we had entered on of these seasons, and that therefore the next day would bably resemble the preceding ten.

Since then all these forms of such an Inductive rule are sible, and we have often no *à priori* grounds for preferring to another, it would seem to be unreasonable to attempt establish any universal formula of anticipation. All that can do is to ascertain what are the circumstances under ich one or other of these rules is, as a matter of fact, nd to be applicable, and to make use of it under those umstances.

VII

§ 31. (VII.) In the cases discussed in (V.) the almost nitely small chances with which we were concerned were rightly neglected from all practical consideration, ho proper it might be, on speculative grounds, to keep our open to their actual existence. But it has often occur me that there is a common error in neglecting to take them account when they may, though individually small, mak for their minuteness by their number. As the mathemat would express it, they may occasionally be capable of *integrated* into a finite or even considerable magnitude

For instance, we may be confronted with a difficulty of which there appears to be only one appreciably po mode of escape. The attempt is made to force us accepting this, however great the odds apparently are ag it, on the ground that improbable as it may seem, it any rate vastly more probable than any of the other can quite admit that, on practical grounds, we may often it reasonable to adopt this course; for we can only ac one supposition, and we naturally and rightly choose, of a quantity of improbabilities, the least improbable. when we are not forced to act, no such decisive preferen demanded of us. It is then perfectly reasonable to re assent to the proposed explanation; even to say distin that we do not believe it, and at the same time to dec at present, to accept any other explanation. We remain fact, in a state of suspense of judgment, a state perf right and reasonable so long as no action demanding a cific choice is forced upon us. One alternative may decidedly probable as compared with any other individu but decidedly improbable as compared with all others lectively. This in itself is intelligible enough; what pe often fail to see is that there is no necessary contradic between saying and feeling this, and yet being prep vigorously to act, when action is forced upon us, as tho *this* alternative were really the true one.

§ 32. To take a specific instance, this way of regarding matter has often occurred to me in disputes upon 'Spi- alist' manifestations. Assent is urged upon us because, is said, no other possible solution can be suggested. It y be quite true that apparently overwhelming difficulties y lie as against each separate alternative solution; but is always sufficiently realized how numerous such solutions y be? No matter that each individually may be almost redible: they ought all to be massed together and thrown o the scale against the proffered solution, when the only estion asked is, Are we to accept this solution? There is unfairness in such a course. We are perfectly ready to pt the same plan against any other individual alterna- e, whenever any person takes to claiming this as *the* ution of the difficulty. We are looking at the matter m a purely logical point of view, and are quite willing, so , to place every solution, spiritualist or otherwise, upon the ne footing. The partisans of every alternative are in newhat the same position as the members of a deliberative embly, in which no one will support the motion of any ler member. Every one can aid effectively in rejecting ery other motion, but no one can succeed in passing his n. Pressure of urgent necessity may possibly force them t of this state of practical inaction, by, so to say, breaking rough the opposition at some point of least resistance; but less aided by some such pressure they are left in a state of peless dead-lock.

§ 33. Assuming that the spiritualistic solution admits and is to receive, scientific treatment, this, it seems to e, is the conclusion to which one might sometimes be led the face of the evidence offered. We might have to say to ery individual explanation, It is incredible, I cannot accept ; and unless circumstances should (which it is hardly possible that they should) force us to a hasty decision,— sion, remember, which need indicate no preference judgment beyond what is just sufficient to turn the sca favour as against any other single alternative,—we le matter thus in abeyance. It will very likely be urg one of the explanations (assuming that all the possib had been included) must be true; this we readily It will probably also be urged that (on the often- principle of Butler) we ought forthwith to accept t which, as compared with the others, is the most pl whatever its absolute worth may be. This seems di an error. To say that such and such an explanation one we should accept, *if* circumstances compelled us t cipate our decision, is quite compatible with its rejection. The only rational position surely is t admitting that the truth is somewhere amongst the alternatives, but confessing plainly that we have n preference for one over another as to permit our sayi thing else than that we disbelieve each one of them.

III § 34. (VIII.) The very common fallacy of 'judg the event,' as it is generally termed, deserves passing here, as it clearly belongs to Probability rather than to though its nature is so obvious to those who have gras general principles of our science, that a very few w remark will suffice. In one sense every propositio consent to be judged by the event, since this is me other words, submitting it to the test of experience there is the widest difference between the test appr to a universal proposition and that appropriate to a proportional or statistical one. The former is subvert single exception; the latter not merely admits exc but implies them. Nothing, however, is more commo to blame advice (in others) because it has happened t unfortunately, or to claim credit for it (in oneself) because has happened to succeed. Of course if the conclusion was owedly one of a probable kind we must be prepared with mplacency to accept a hostile event, or even a succession of em; it is not until the succession shows a disposition to ntinue over long that suspicion and doubt should arise, and en only by a comparison of the degree of the assigned obability, and the magnitude of the departure from it ich experience exhibits. For any single failure the reply ust be, 'the advice was sound' (supposing, that is, that it is to be justified in the long run), 'and I shall offer it again der the same circumstances.'

§ 35. The distinction drawn in the above instance serves careful consideration; for owing to the wide differ- ce between the kind of propositions dealt with in Proba- ity and in ordinary Logic, and the consequent difference in e nature of the proof offered, it is quite possible for argu- nts of the same general appearance to be valid in the mer and fallacious in the latter, and conversely.

For instance, take the well-known fallacy which consists simply converting a universal affirmative, *i.e.* in passing m All A is B to All B is A. When, as in common Logic, e conclusion is to be as certain as the premise, there is not vord to be said for such a step. But if we look at the ocess with the more indulgent eye of Induction or Proba- lity we see that a very fair case may sometimes be made t for it. The mere fact that 'Some B is A' raises a cer- in presumption that any particular B taken at random will an A. There is some reason, at any rate, for the belief, ugh in the absence of statistics as to the relative fre- ency of A and B we are unable to assign a value to this lief. I suspect that there may be many cases in which a n has inferred that some particular B is an A on the

ground that All *A* is *B*, who might justly plead in his be
that he never meant it to be a necessary, but only a
bable inference. The same remarks will of course ap
also to the logical fallacy of Undistributed Middle.

Now for a case of the opposite kind, *i.e.* one in wh
Probability fails us, whereas the circumstances seem clo
analogous to those in which ordinary inference would
able to make a stand. Suppose that I know that one let
in a million is lost when in charge of the post. I write t
friend and get no answer. Have I any reason to supp
that the fault lies with him? Here is an event (viz.
loss of the letter) which has certainly happened; and we s
pose that, of the only two causes to which it can be assign
the 'value,' *i.e.* statistical frequency, of one is accurat
assigned, does it not seem natural to suppose that someth
can be inferred as to the likelihood that the other cause h
been operative? To say that nothing can be known abo
its adequacy under these circumstances looks at first sig
like asserting that an equation in which there is only o
unknown term is theoretically insoluble.

As examples of this kind have been amply discussed
the chapter upon Inverse rules of Probability I need do
more here than remind the reader that no conclusion wh
ever can be drawn as to the likelihood that the fault l
with my friend rather than with the Post Office. Unless
either know, or make some assumption about, the frequen
with which he neglects to answer the letters he receives, t
problem remains insoluble.

The reason why the apparent analogy, indicated abo
to an equation with only one unknown quantity, fails to ho
good, is that for the purposes of Probability there are rea
two unknown quantities. What we deal with are propo
tional or statistical propositions. Now we are only told th

n the instance in question the letter was lost, not that they ere found to be lost in such and such a proportion of cases. ad this latter information been given to us we should really ave had but one unknown quantity to determine, viz. the lative frequency with which my correspondent neglects to nswer his letters, and we could then have determined this ith the greatest ease.

CHAPTER XV.

INSURANCE AND GAMBLING.

§ 1. If the reader will recall to mind the fundam postulate of the Science of Probability, established an plained in the first few chapters, and so abundantly trated since, he will readily recognize that the two op characteristics of individual irregularity and average larity will naturally be differently estimated by diff minds. To some persons the elements of uncertainty be so painful, either in themselves or in their conseque that they are anxious to adopt some means of dimini them. To others the ultimate regularity of life, at an within certain departments, its monotony as they consid may be so wearisome that *they* equally wish to effect alteration and improvement in its characteristics. We discuss briefly these mental tendencies, and the most si and obvious modes of satisfying them.

To some persons, as we have said, the world is all to of change and irregularity and consequent uncertainty. lization has done much to diminish these characteristi certain directions, but it has unquestionably aggravated in other directions, and it might not be very easy to say certainty in which of these respects its operation has bee present, on the whole most effective. The diminutio irregularity is exemplified, amongst other things, in the of the staple products which supply our necessary food hing. With respect to them, famine and scarcity are by iparison almost unknown now, at any rate in tolerably lized communities. As a consequence of this, and of the t improvements in the means of transporting goods and veying intelligence, the fluctuations in the price of such cles are much less than they once were. In other direc- is, however, the reverse has been the case. Fashion, for ance, now induces so many people in every large com- nity simultaneously to desire the same thing, that great tuations in value may ensue. Moreover a whole group of ses (to enter upon any discussion of which would be to ich upon the ground of Political Economy) combine to luce great and frequent variations in matters concerning lit and the currency, which formerly had no existence. ikruptcy, for instance, is from the nature of the case, ost wholly a creation of modern times. We will not at- ipt to strike any balance between these opposite results nodern civilization, beyond remarking that in matters of ne importance the actual uncertainties have been pro- ly on the whole diminished, whereas in those which affect pocket rather than the life, they have been rather in- ised. It might also be argued with some plausibility t in cases where the actual uncertainties have not become ater, they have for all practical purposes done so, by their sequences frequently becoming more serious, or by our mate of these consequences becoming higher.

§ 2. However the above question, as to the ultimate ance of gain or loss, should be decided, there can be no ibt that many persons find the present amount of uncer- ity in some of the affairs of life greater than suits their te. How are they to diminish it? Something of course y be done, as regards the individual cases, by prudence l foresight. Our houses may be built with a view not to take fire so readily, or precautions may be taken that shall be fire-engines at hand. In the warding off of from disease and accident, something may be done by one who chooses to live prudently. Precautions of the kind, however, do not introduce any questions of Proba These latter considerations only come in when we be invoke the regularity of the average to save us fro irregularities of the details. We cannot, it is true, r the uncertainty in itself, but we can so act that the quences of that uncertainty shall be less to us, or to th whom we are interested. Take the case of Life Insu A professional man who has nothing but the income he to depend upon, knows that the whole of that incom vanish in a moment by his death. This is a state of which he cannot prevent; and if he were the only such a position, or were unable or unwilling to combin his fellow-men, there would be nothing more to be d the matter except to live within his income as much a sible, and so leave a margin of savings.

§ 3. There is however an easy mode of escape fo All that he has to do is to agree with a number of who are in the same position as himself, to make up say, a common purse. They may resolve that those o number who live to work beyond the average length shall contribute to support the families of those wh earlier. If a few only concurred in such a resolutio would not gain very much, for they would still be re by but a slight step from that uncertainty which th seeking to escape. What is essential is that a consid number should thus combine so as to get the benefit comparative regularity which the average, as is well l almost always tends to exhibit.

§ 4. The above simple considerations really conta essence of all insurance. Such points as the fact that the agreement for indemnity extends only to a certain definite sum of money; and that instead of calling for an occasional general contribution at the time of the death of each member they substitute a fixed annual premium, out of the proceeds of which the payment is to be made, are merely accidents of convenience and arrangement. Insurance is simply equivalent to a mutual contract amongst those who dread the consequences of the uncertainty of their life or employment, that they will employ the aggregate regularity to neutralize as far as possible the individual irregularity. They know that for every one who gains by such a contract another will lose as much; or if one gains a great deal many must have lost a little. They know also that hardly any of their number can expect to find the arrangement a 'fair' one, in the sense that they just get back again what they have paid in premiums, after deducting the necessary expenses of management; but they deliberately prefer this state of things. They consist of a body of persons who think it decidedly better to leave behind them a comparatively fixed fortune, rather than one which is extremely uncertain in amount; although they are perfectly aware that, owing to the unavoidable expenses of managing the affairs of such a society, the comparatively fixed sum, so to be left, will be a trifle less than the average fortunes which would have been left had no such system of insurance been adopted.

As this is not a regular treatise upon Insurance no more need be said upon the exact nature of such societies, beyond pointing out that they are of various different kinds. Sometimes they really are what we have compared them with, viz. mutual agreements amongst a group of persons to make up each other's losses to a certain extent. Into this category fall the Mutual Insurance Societies, Benefit Societies, Trades

Unions (in respect of some of their functions), together with innumerable other societies which go by various names. Sometimes they are companies worked by proprietors or shareholders for a profit, like any other industrial enterprise. This is the case, I believe, with the majority of the ordinary Life Insurance Societies. Sometimes, again, it is the State which undertakes the management, as in the case of our Post Office Insurance business.

§ 5. It is clear that there is no necessary limit to the range of application of this principle[1]. It is quite conceivable that the majority of the inhabitants of some nation might be so enamoured of security that they should devise a grand insurance society to cover almost every concern in life. They could not indeed abolish uncertainty, for the conditions of life are very far from permitting this, but they could without much difficulty get rid of the worst of the *consequences* of it. They might determine to insure not merely their lives, houses, ships, and other things in respect of which sudden and total loss is possible, but also to insure their business; in the sense of avoiding not only ankruptcy, but even casual bad years, on the same principle f commutation. Unfamiliar as such an aim may appear when introduced in this language, it is nevertheless one which under a name of suspicious import to the conservative lasses has had a good deal of attention directed to it. It is eally scarcely anything else than Communism, which might ndeed be defined as a universal and compulsory[1] insurance ociety which is to take account of all departments of business, and, in some at least of its forms, to invade the province of social and domestic life as well.

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[1] The question of the advisability of inoculation against the small-pox, which gave rise to much discussion amongst the writers on Probability during the last century, is a case in point of the same principles applied to a very different kind of instance. The loss against which the insurance was directed was death by small-pox, the premium paid was the illness and other inconvenience, and the very small risk of death, from the inoculation. The disputes which thence arose amongst writers on the subject involved the same difficulties as to the balance between certain moderate loss and contingent great loss. In the seventeenth century it seems to have been an occasional practice, before a journey into the Mediterranean, to insure against capture by Moorish pirates, with a view to secure having the ransom paid. (See, for an account of some extraordinary developments of the insurance principle, Walford's *Insurance Guide and Handbook.* It is not written in a very scientific spirit, but it contains much information on all matters connected with insurance.)

Although nothing so comprehensive as this is likely to be practically carried out on any very large scale, it deserves notice that the principle itself is steadily spreading in every direction in matters of detail. It is, for instance, the great omplaint against Trades Unions that they too often seek to ecure these results in respect of the equalization of the workmen's wages, thus insuring to some degree against incompetence, as they rightly and wisely do against illness and oss of work. Again, there is the Tradesman's Mutual Proection Society, which insures against the occasional loss ntailed by the necessity of having to conduct prosecutions t law. There are societies in many towns for the prose-ution of petty thefts, with the object of escaping the same uncertain and perhaps serious loss. Amongst instances of nsurance *for* the people rather than *by* them, there is of ourse the giant example of the English Poor Law, in which the resemblance to an initial Communistic system becomes very marked. The poor are insured against loss of work arising not only from illness and old age, but f any cause except wilful idleness. They do not, it is t pay the whole premium, but since they mostly bear s portion of the burden of municipal and county taxa they must certainly be considered as paying a part the premium. In some branches also of the public private services the system is adopted of deducting a centage from the wage or salary, for the purpose o semi-compulsory insurance against death, illness or su annuation.

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[1] All that is meant by the above omparison is that the ideal aimed t by Communism is similar to that f Insurance. If we look at the rocesses by which it would be arried out, and the means for enforcing it, the matter would of course assume a very different aspect. Similarly with the action of Trades Unionism referred to in the next paragraph.

§ 6. Closely connected with Insurance, as an applica of Probability, though of course by contrast, stands Gambl Though we cannot, in strictness, term either of these p tices the converse of the other, it seems nevertheless cor to say that they spring from opposite mental tenden Some persons, as has been said, find life too monotonous their taste, or rather the region of what can be predi with certainty is too large and predominant in their esti tion. They can easily adopt two courses for securing changes they desire. They may, for one thing, aggra and intensify the results of events which are comparati incapable of prevision, these events not being in themse of sufficient importance to excite any strong emotions. most obvious way of doing this is by betting upon th Or again, they may invent games or other pursuits, individual contingencies of which are entirely removed all possible human prevision, and then make heavy m consequences depend upon these contingencies. Thi gambling proper, carried on mostly by means of cards dice and the roulette.

The gambling spirit, as we have said, seeks for the citement of uncertainty and variety. When therefore pe *make* a long continued practice of playing, especially if akes for which they play are moderate in comparison with eir fortune, this uncertainty from the nature of the case egins to diminish. The thoroughly practised gambler, if e possesses more than usual skill (in games where skill unts for something), must be regarded as a man following profession, though a profession for the most part of a risky d exciting kind, to say nothing of its ignoble and often ishonest character. If, on the other hand, his skill is below e average, or the game is one in which skill does not tell d the odds are slightly in favour of his antagonist, as in e gaming tables, one light in which he can be regarded that of a man who is following a favourite amusement; this amusement involves a constant annual outlay on his rt, that is nothing more than what has to be said of most her amusements.

§ 7. We cannot, of course, give such a rational expla-tion as the above in every case. There are plenty of vices, and plenty of fanatics, who go on steadily losing in e full conviction that they will eventually come out win-ers. But it is hard to believe that such ignorance, or such tellectual twist, can really be so widely prevalent as would requisite to constitute them the rule rather than the ex-ption. There must surely be some very general impulse hich is gratified by such resources, and it is not easy to see hat else this can be than a love of that variety and con-quent excitement which can only be found in perfection here exact prevision is impossible.

It is of course very difficult to make any generalization re as to the comparative prevalence of various motives ongst mankind; but when one considers what is the fference which most quiet ordinary whist players feel tween a game for 'love' and one in which there is a all stake, one cannot but assign a high value to the

influence of a wish to emphasize the excitement o
and gain.

I would not for a moment underrate the practical da
which are found to attend the practice of gamblin
is remarked that the gambler, if he continues to play
long time, is under an almost irresistible impulse to inc
his stakes, and so re-introduce the element of uncert
It is in fact this tendency to be thus led on, which
the principal danger and mischief of the practice. Ris
uncertainty are still such normal characteristics of even
lized life, that the mere extension of such tendencies
new fields does not in itself offer any very alarming
pect. It is only to be deprecated in so far as there
danger, which experience shows to be no trifling one
the fascination found in the pursuit should lead men
following it up into excessive lengths[1].

§ 8. The above general treatment of Gambling
Insurance seems to me the only rational and sound
ciple of division;—namely, that on which the different
tices which, under various names, are known as gam
or insurance, are arranged in accordance with the spi
which they are the outcome, and therefore of the r
which they are designed to secure. If we were to att

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[1] One of the best discussions that I have recently seen on these subjects, by a writer at once thoroughly competent and well informed, is in Mr Proctor's *Chance and Luck.* It appears to me however that he runs into an extreme in his denunciation not of the folly but of the dishonesty of all gambling. Surely also it is a strained use of language to speak of all lotteries as 'unfair' and even '*swindling*' on the ground that the sum-total of what they distri
prizes is less than that of wh
receive in payments. The diff
in respect of information deli
ly withheld and false reports
spread, between most of the l
that have been supported, a
bubble companies which jus
serve the name of swindles, o
prevent the same name being
to both.

judge and arrange them according to the names which ey currently bear, we should find ourselves led to no d of systematic division whatever; the fact being that ce they all alike involve, as their essential characteristic, yments and receipts, one or both of which are necessarily certain in their date or amount, the names may often be erchanged.

For instance, a lottery and an ordinary insurance society inst accident, if we merely look to the processes per- med in them, are to all intents and purposes identical. each alike there is a small payment which is certain in ount, and a great receipt which is uncertain in amount. great many persons pay the small premium, whereas a w only of their number obtain a prize, the rest getting return whatever for their outlay. In each case alike, o, the aggregate receipts and losses are intended to lance each other, after allowing for the profits of those o carry on the undertaking. But of course when we ke into account the occasions upon which the insurers t their prizes, we see that there is all the difference in the rld between receiving them at haphazard, as in a lottery, d receiving them as a partial set-off to a broken limb or ured constitution, as in the insurance society.

Again, the language of betting may be easily made to ver almost every kind of insurance. Indeed De Morgan described life insurance as a bet which the individual kes with the company, that he will not live beyond a tain age. If he dies young, he is pecuniarily a gainer, if dies late he is a loser[1]. Here, too, though the expression is technically quite correct (since any such deliberate ri money, upon an unproductive venture, may fall under definition of a bet), there is the broadest distinction bet betting with no other view whatever than that of ri money, and betting with the view of diminishing risk loss as much as possible. In fact, if the language of spo life is to be introduced into the matter, we ought, I pres to speak of the insurer as 'hedging' against his death.

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[1] "A fire insurance is a simple between the office and the party, a life insurance is a collection agers. There is something of principle of a wager in every transaction in which the results of a future event are to bring gain or loss." *Penny Cyclopædia*, under the head of *Wager*.

§ 9. Again, in Tontines we have a system of wh often called Insurance, and in certain points rightly but which is to all intents and purposes simply and d lutely a gambling transaction. They have been enti abandoned, I believe, for some time, but were once m popular, especially in France. On this plan the State whatever society manages the business, does not gain thing until the last member of the Tontine is dead. A number of the survivors diminishes, the same sum-tot annuities still continues to be paid amongst them, as lo any are left alive, so that each receives a gradually incre sum. Hence those who die early, instead of receiving most, as on the ordinary plan, receive the least; for a death of each member the annuity ceases absolutely, as he and his relations are concerned. The whole therefore is to all intents and purposes a gigantic sy of betting, to see which can live the longest; the being the common stake-holder, and receiving a heavy mission for its superintendence, this commission being ally its sole motive for encouraging such a transaction. recorded of one of the French Tontines[1] that a widow was left, as the last survivor, to receive an annuity of 7 livres during the rest of the life which she could manag drag on after that age;—she having originally subscril gle sum of 300 livres only. It is obvious that such a system this, though it may sometimes go by the name of insur- ce, is utterly opposed to the spirit of true insurance, ce it tends to aggravate existing inequalities of fortune tead of to mitigate them. The insurer here bets that will die old; in ordinary insurance he bets that he will young.

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[1] *Encyclopédie Methodique*, under the head of Tontines.

Again, to take one final instance, common opinion often gards the bank or company which keeps a *rouge et noir* ble, and the individuals who risk their money at it, as being th alike engaged in gambling. So they may be, tech- ally, but for all practical purposes such a bank is as sure d safe a business as that of any ordinary insurance society, d probably far steadier in its receipts than the majority of dinary trades in a manufacturing or commercial city. The nk goes in for many and small transactions, in proportion its capital; their customers, very often, in proportion to eir incomes go in for very heavy transactions. That the rmer comes out a gainer year after year depends, of course, pon the fact that the tables are notoriously slightly in their vour. But the *steadiness* of these gains when compared ith the unsteadiness of the individual losses depends simply pon,—in fact, is merely an illustration of,—the one great ermanent contrast which lies at the basis of all reasoning Probability.

§ 10. We have so far regarded Insurance and Gambling being each the product of a natural impulse, and as having ch, if we look merely to experience, a great mass of human dgment in its favour. The popular moral judgment, however, hich applauds the one and condemns the other rests in great rt upon an assumption, which has doubtless much truth it, but which is often interpreted with an absoluteness which ads to error in each direction;—the duty of insurance being too peremptorily urged upon every one, and the prac gambling too universally regarded as involving a sacri real self-interest, as being in fact little better than sistent blunder. The assumption in question seems extracted from the acknowledged advantages of insu and then invoked to condemn the practice of gambling. in so doing the fact does not seem to be sufficiently nized that the latter practice, if we merely look to t tent and antiquity of the tacit vote of mankind in its f might surely claim to carry the day.

It is of course obvious that in all cases with whi are concerned, the aggregate wealth is unaltered; being merely transferred from one person to another. loss of one is precisely equivalent to the gain of an At least this is the approximation to the truth with whi find it convenient to start[1]. Now if the happiness wh yielded by wealth were always in direct proportion amount, it is not easy to see why insurance should be cated or gambling condemned. In the case of the latt is obvious enough. I have lost £50, say, but others (more as the case may be) have gained it, and the in of their happiness would exactly balance the diminut mine. In the case of Insurance there is a slight com tion, arising from the fact that the falling in of the does not happen at random (otherwise, as already p t, it would be simply a lottery), but is made contingent pon some kind of loss, which it is intended as far as possible balance. I insure myself on a railway journey, break my g in an accident, and, having paid threepence for my cket, receive say £200 compensation from the insurance mpany. The same remarks, however, apply here; the ppiness I acquire by this £200 would only just balance the gregate loss of the 16,000 who have paid their threepences d received no return for them, were happiness always rectly proportional to wealth.

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[1] Of course, if we introduce considerations of Political Economy, corrections will have to be made. For one thing, every Insurance Office is, as De Morgan repeatedly insists, a *Savings Bank* as well as an Insurance Office. The Office invests the premiums, and can therefore afford to pay a larger sum than would otherwise be the case. Ag the case of gambling, a large capital by any one will almos sarily involve an actual dest of wealth; to say nothing fact that, practically, gambli causes a constant transfer of from productive to unproduct poses.

§ 11. The practice of Insurance does not, I think, give e to many questions of theoretic interest, and need not erefore detain us longer. The fact is that it has hardly t been applied sufficiently long and widely, or to matters hich admit of sufficiently accurate statistical treatment, cept in one department. This, of course, is Life Insurance; t the subject is one which requires constant attention to tails of statistics, and is (rightly) mainly carried out in ict accordance with routine. As an illustration of this e need merely refer to the works of De Morgan,—a profes- nal actuary as well as a writer on the theory of Probability, -who has found but little opportunity to aid his speculative eatment of Probability by examples drawn from this class considerations.

With Gambling it is otherwise. Not only have a variety interesting single problems been discussed (of which the etersburg problem is the best known) but several specula- ve questions of considerable importance have been raised. ne of these concerns the disadvantages of the practice of mbling. There have been a number of writers who, not ntent with dwelling upon the obvious moral and indirect ischief which results, in the shape of over-excitement, nsequent greed, withdrawal from the steady business

habits which alone insure prosperity in the long run, di
sion of wealth into dishonest hands, &c., have endeavou
to demonstrate the necessary loss caused by the practice.

§ 12. These attempts may be divided into two clas
There are (1) those which appeal to merely numerical c
siderations, and (2) those which introduce what is called
'moral' as distinguished from the mathematical value o
future contingency.

(1) For instance, an ingenious attempt has been m
by Mr Whitworth to prove that gambling is necessa
disadvantageous on purely mathematical grounds.

When two persons play against each other one of the
must be ruined sooner or later, even though the game b
fair one, supposing that they go on playing long enough;
one with the smaller income having of course the w
chance of being the lucky survivor. If one of them ha
finite, and the other an infinite income, it must clearly
the former who will be the ultimate sufferer if they go
long enough. It is then maintained that this is in fact ev
individual gambler's position, "no one is restricted to gam
ling with one single opponent; the speculator deals with t
public at large, with a world whose resources are practica
unlimited. There is a prospect that his operations m
terminate to his own disadvantage, through his having
thing more to stake; but there is no prospect that it w
terminate to his advantage through the exhaustion of t
resources of the world. Every one who gambles is carryi
on an unequal warfare: he is ranged with a restricted capi
against an adversary whose means are infinite[1]."

In the above argument it is surely overlooked that t
adversaries against whom he plays are not one body wit
common purse, like the bank in a gambling establishme

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[1] *Choice and Chance*, Ed. II. p. 208.

each of these adversaries is in exactly the same position as he himself is, and a precisely similar proof might be employed to show that each of them must be eventually ruined which is of course a reduction to absurdity. Gambling can only transfer money from one player to another, and therefore none of it can be actually lost.

§ 13. What really becomes of the money, when they play to extremity, is not difficult to see. First suppose a limited number of players. If they go on long enough, the money will at last all find its way into the pocket of some one of their number. If their fortunes were originally equal, each stands the same chance of being the lucky survivor; in which case we cannot assert, on any numerical grounds, that the prospect of the play is disadvantageous to any one of them. If their fortunes were unequal, the one who had the largest sum to begin with can be shown to have the best chance, according to some assignable law, of being left the final winner; in which case it must be just as advantageous for him, as it was disadvantageous for his less wealthy competitors.

When, instead of a limited number of players, we suppose an unlimited number, each as he is ruined retiring from the table and letting another come in, the results are more complicated, but their general tendency can be readily distinguished. If we supposed that no one retired except when he was ruined, we should have a state of things in which all the old players were growing gradually richer. In this case the prospect before the new comers would steadily grow worse and worse, for their chance of winning against such rich opponents would be exceedingly small. But as this is an unreasonable supposition, we ought rather to assume that not only do the ruined victims retire, but also that those who have gained fortunes of a certain amount retire also, so that the aggregate and average wealth of gambling body remains pretty steady. What chance given player has of being ruined, and how long he expect to hold out before being ruined, will depend of upon the initial incomes of the players, the rules of game, the stakes for which they play, and other consid tions. But it is clear that for all that is lost by one, precisely equal sum must be gained by others, and therefore any particular gambler can only be cautioned forehand that his conduct is not to be recommended, appealing to some such suppositions as those already tioned in a former section.

§ 14. As an additional justification of this view reader may observe that the state of things in the example is one which, expressed in somewhat diffe language and with a slight alteration of circumstances, being incessantly carried on upon a gigantic scale up every side of us. Call it the competition of merchants traders in a commercial country, and the general results familiar enough. It is true that in so far as skill comes the question, they are not properly gamblers; but in so as chance and risk do, they may be fairly so termed, and many branches of business this must necessarily be the to a very considerable extent. Whenever business is carri on in a reckless way, the comparison is on general gro fair enough. In each case alike we find some retiring rui and some making their fortunes; and in each case also the chances, *cæteris paribus*, lie with those who the largest fortunes. Every one is, in a sense, strugg against the collective commercial world, but since each of competitors is doing the same, we clearly could not cauti any of them (except indeed the poorer ones) that their effor must finally end in disadvantage.

§ 15. If we wish to see this result displayed in its most decisive form we may find a good analogy in a very different class of events, viz. in the fate of *surnames*. We are all gamblers in this respect, and the game is carried out to the last farthing with a rigour unknown at Newmarket or Monte Carlo. In its complete treatment the subject is a very intricate one[1], but a simple example will serve to display the general tendency. Suppose a colony comprising 1000 couples of different surnames, and suppose that each of these has four children who grow up to marry. Approximately, one in 16 of these families will consist of girls only; and therefore, under ordinary conventions, about 62 of the names will have disappeared for ever after the next generation. Four again out of 16 will have but one boy, each of whom will of course be in the same position as his father, viz. the sole representative of his name. Accordingly in the next generation one in 16 of these names will again drop out, and so the process continues. The number which disappears in each successive generation becomes smaller, as the stability of the survivors becomes greater owing to their larger numbers. But there is no check to the process.

§ 16. The analogy here is a very close one, the names which thus disappear corresponding to the gamblers who etire ruined and those which increase in number corre-ponding to the lucky winners. The ultimate goal in each ase alike,—of course an exceedingly remote one,—is the exclusive survival of one at the expense of all the others. That one surname does thus drop out after another must have struck every one who has made any enquiry into family genealogy, and various fanciful accounts have been given by those unfamiliar with the theory of probability. What is often apt to be overlooked is the extreme slightness of what may be termed the "turn of the tables" in favour of the survival at each generation. In the above numerical example we have made an extravagantly favourable supposition, by assuming that the population doubles at every generation. In an old and thickly populated country where the numbers increase very slowly, we should be much nearer the mark in assuming that the average effective family,—that is, the average number of children who live to marry,—was only *two*. In this case every family which was represented at any time by but a single male would have but three chances in four of surviving extinction, and of course the process of thinning out would be a more rapid one.

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[1] It was, I believe, first treated as serious problem by Mr Galton. See the *Journal Anthrop. Inst.* Vol. IV. 1875, where a complete mathematical solution is indicated by Mr H. W. Watson.)

§ 17. The most interesting class of attempts to prove the disadvantages of gambling appeal to what is technically called 'moral expectation' as distinguished from 'mathematical expectation.' The latter may be defined simply as the average money value of the venture in question; that is, it is the product of the amount to b gained (or lost) and the chance of gaining (or losing) it For instance, if I bet four to one in sovereigns against th occurrence of ace with a single die there would be, on th average of many throws, a loss of four pounds against a gai of five pounds on each set of six occurrences; i.e. ther would be an average gain of three shillings and fourpen on each throw. This is called the true or mathemati expectation. The so-called 'moral expectation', on the oth hand, is the subjective value of this mathematical expe tation. That is, instead of reckoning a money fortune the ordinary way, as what it *is*, the attempt is made *reckon* it at what it is *felt* to be. The elements of comp ı therefore become, not pounds and shillings, but sums easure enjoyed actually or in prospect. Accordingly reckoning the present value of a future gain, we must multiply, not the objective but the subjective value, e chance we have of securing that gain.

/ith regard to the exact relation of this moral fortune e physical various more or less arbitrary assumptions been made. One writer (Buffon) considers that the l value of any given sum varies inversely with the wealth of the person who gains it. Another (D. Berı) starting from a different assumption, which we shall ntly have to notice more particularly, makes the moral of a fortune vary as the logarithm of its actual amount[1]. rd (Cramer) makes it vary with the square root of the nt.

18. Historically, these proposals have sprung from wish to reconcile the conclusions of the Petersburg em with the dictates of practical common sense; for, ıbstituting the moral for the physical estimate the value of the expectation could be reduced to a finite On this ground therefore such proposals have no great əst, for, as we have seen, there is no serious difficulty in roblem when rightly understood.

hese same proposals however have been employed in

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rnoulli himself does not seem e based his conclusions upon experience. But it is a note- fact that the assumption with he starts, viz. that the sub- value of any small increment inversely proportional to the hen possessed (x), and which t once to the logarithmic law mentioned, is identical with ıich is now familiar enough to every psychologist. It is what is commonly called Fechner's Law, which he has established by aid of an enormous amount of careful experiment in the case of a number of our simple sensations. But I do not believe that he has made any claim that such a law holds good in the far more intricate dependence of happiness upon wealth.

order to prove that gambling is necessarily disadvantageous, and this to both parties. Take, for instance, Bernoulli's supposition. It can be readily shown that if two persons each with a sum of £50 to start with choose to risk, say, £10 upon an even wager there will be a loss of happiness as a result; for the pleasure gained by the possessor of £60 will not be equal to that which is lost by the man who leaves off with £40[1].

§ 19. This is the form of argument commonly adopted; but, as it stands, it does not seem conclusive. It may surely be replied that all which is thus proved is that *inequality* is bad, on the ground that two fortunes of £50 are better than one of £60 and one of £40. Conceive for instance that the original fortunes had been £60 and £40 respectively, the event may result in an increase of happiness; for this will certainly be the case if the richer man loses and the fortunes are thus equalized. This is quite true; and we are therefore obliged to show,—what can be very easily shown,—that if the other alternative had taken place and the two fortunes had been made still more unequal (viz. £65 and £35 respectively) the happiness thus lost would more than balance what would have been gained by the equalization. And since these two suppositions are equally likely there will be a loss in the long run.

The consideration just adduced seems however to show

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[1] The formula expressive of this moral happiness is $c \log \frac{x}{a}$; where x stands for the physical fortune possessed at the time, and a for that small value of it at which happiness is supposed to disappear: c being an arbitrary constant. Let two persons, whose fortune is x, risk y on an even bet. Then the balance, as regards happiness, must be drawn between

$$c \log \frac{x}{a} \text{ and } \tfrac{1}{2} c \log \frac{x+y}{a} + \tfrac{1}{2} c \log \frac{x-y}{a},$$

or $\log x^2$ and $\log (x+y)(x-y)$,

or x^2 and $x^2 - y^2$,

the former of which is necessarily the greater.

he common way of stating the conclusion is rather ding; and that, on the assumption in question as law of dependence of happiness on wealth, it really is ase that the effective element in rendering gambling antageous is its tendency to the increase of the in- ty in the distribution of wealth.

20. This raises two questions, one of some speculative st in connection with our subject, and the other of me importance in the conduct of life. The first is this: apart from any particular assumption which we make moral fortunes or laws of variation of happiness, is fact that gambling tends to increase the existing alities of wealth? Theoretically there is no doubt that so. Take the simplest case and suppose two people g for a pound. If their fortunes were equal to begin there must be resultant inequality. If they were al there is an even chance of the inequality being sed or diminished; but since the increase is propor- ly greater than the decrease, the final result remains of me kind as when the fortunes were equal[1]. Taking a general view the same conclusion underlies all our ing as to the averages of large numbers, viz. that the ant divergencies increase absolutely (however they ish relatively) as the numbers become greater. And of we refer to these absolute divergencies when we lking of the distribution of wealth.

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is may be seen more clearly ows. Suppose two pair of rs, each pair consisting of ossessing £50 and £30 re- ely. Now if we suppose the man to win in one case and orer in the other these two will be a fair representation of the average; for there are only two alternatives and these will be equally frequent in the long run. It is obvious that we have had two fortunes of £50 and two of £30 converted into one of £20, two of £40, and one of £60. And this is clearly an increase of inequality.

§ 21. This is the theoretic conclusion. How far t actual practice of gambling introduces counteracting agen must be left to the determination of those who are co petent to pronounce. So far as outsiders are authori to judge from what they read in the newspapers and ot public sources of information, it would appear that th counteracting agencies are very considerable, and that consequence it is a rather insecure argument to adva against gambling. Many a large fortune has notorio been squandered on the race-course or in gambling saloo and most certainly a large portion, if not the major pa has gone to swell the incomes of many who were by compar son poor. But the solution of this question must clearly b left to those who have better opportunities of knowi the facts than is to be expected on the part of writers o Probability.

§ 22. The general conclusion to be drawn is that tho who invoked this principle of moral fortune as an argume against gambling were really raising a much more intrica and far-reaching problem than they were aware of. Wh they were at work upon was the question, What is th distribution of wealth which tends to secure the maximu of happiness ? Is this best secured by equality or inequality Had they really followed out the doctrine on which the denunciation of gambling was founded they ought to hav adopted the Socialist's ideal as being distinctly that whic tends to increase happiness. And they ought to hav brought under the same disapprobation which they ex pressed against gambling all those tendencies of mode civilized life which work in the same direction. For i stance; keen competition, speculative operations, extende facilities of credit, mechanical inventions, enlargement o business operations into vast firms:—all these, and oth milar tendencies too numerous to mention here, have had me influence in the way of adding to existing inequalities. ey are, or have been, in consequence denounced by cialists: are we honestly to bring them to this test in der to ascertain whether or not they are to be condemned? he reader who wishes to see what sort of problems this sumption of 'moral fortune' ought to introduce may be commended to read Mr F. Y. Edgeworth's *Mathematical sychics*, the only work with which I am acquainted which eats of these questions.

CHAPTER XVI.

THE APPLICATION OF PROBABILITY TO TESTIMONY.

§ 1. ON the principles which have been adopted in th work, it becomes questionable whether several classes problems which may seem to have acquired a prescripti right to admission, will not have to be excluded from t science of Probability. The most important, perhaps, these refer to what is commonly called the credibility testimony, estimated either at first hand and directly, or influencing a juryman, and so reaching us through h sagacity and trustworthiness. Almost every treatise up the science contains a discussion of the principles accordi to which credit is to be attached to combinations of t reports of witnesses of various degrees of trustworthiness, the verdicts of juries consisting of larger or smaller numbe A great modern mathematician, Poisson, has written elaborate treatise expressly upon this subject; whilst a co siderable portion of the works of Laplace, De Morgan, a others, is devoted to an examination of similar enquiries. would be presumptuous to differ from such authorities these, except upon the strongest grounds; but I confess th the extraordinary ingenuity and mathematical ability whi have been devoted to these problems, considered as questio in Probability, fails to convince me that they ought to ha been so considered. The following are the principal groun for this opinion.

§ 2. It will be remembered that in the course of the pter on Induction we entered into a detailed investiga- n of the process demanded of us when, instead of the ropriate propositions from which the inference was to be de being set before us, the *individual* presented himself, d the task was imposed upon us of selecting the requisite ups or series to which to refer him. In other words, in- ad of calculating the chance of an event from determinate ditions of frequency of its occurrence (these being either ined by direct experience, or deductively inferred) we e to *select* the conditions of frequency out of a plurality more or less suitable ones. When the problem is pre- ted to us at such a stage as this, we may of course assume t the preliminary process of obtaining the statistics ch are extended into the proportional propositions has n already performed; we may suppose therefore that we already in possession of a quantity of such propositions, principal remaining doubt being as to which of them should then employ. This selection was shown to be a certain extent arbitrary; for, owing to the fact of the ividual possessing a large number of different properties, became in consequence a member of different series or ups, which might present different averages. We must examine, somewhat more fully than we did before, practical conditions under which any difficulty arising m this source ceases to be of importance.

§ 3. One condition of this kind is very simple and ob- us. It is that the different statistics with which we are ented should not in reality offer materially different lts. If, for instance, we were enquiring into the pro- bility of a man aged forty dying within the year, we might we pleased take into account the fact of his having red ir, or his having been born in a certain county or town.

Each of these circumstances would serve to specializ individual, and therefore to restrict the limits of the stat which were applicable to his case. But the considerati such qualities as these would either leave the average cisely as it was, or produce such an unimportant alter in it as no one would think of taking into account. Th we could hardly say with certainty of any concei characteristic that it has absolutely no bearing on result, we may still feel very confident that the bearin such characteristics as these is utterly insignificant. course in the extreme case of the things most perf suited to the Calculus of Probability, viz. games of chance, these subsidiary characteristics are quite irrele Any further particulars about the characteristics of the in a really fair pack, beyond those which are familiar t the players, would convey no information whatever about result.

Or again; although the different sets of statistics not as above give almost identical results, yet they ma what practically comes to very much the same thing, is, arrange themselves into a small number of groups, a the statistics in any one group practically coinciding in t results. If for example a consumptive man desired to in his life, there would be a marked difference in the stati according as we took his peculiar state of health into acc or not. We should here have two sets of statistics, so cl marked off from one another that they might almost with the distinctions of natural kinds, and which would consequence offer decidedly different results. If we to specialize still further, by taking into account insignifi qualities like those mentioned in the last paragraph, might indeed get more limited sets of statistics applic to persons still more closely resembling the individual

stion, but these would not differ sufficiently in their
lts to make it worth our while to do so. In other words,
different propositions which are applicable to the case in
nt arrange themselves into a limited number of groups,
ch, and which only, need be taken into account; whence
range of choice amongst them is very much diminished
ractice.

§ 4. The reasons for the conditions above described are
difficult to detect. Where these conditions exist the
cess of selecting a series or class to which to refer any
ividual is very simple, and the selection is, for the par-
lar purposes of inference, final. In any case of insurance,
example, the question we have to decide is of the very
ple kind; Is *A.B.* a man of a certain age? If so one in
y in his circumstances will die in the course of the year.
ny further questions have to be decided they would be of
following description. Is *A.B.* a healthy man? Does he
ow a dangerous trade? But here too the classes in
stion are but few, and the limits by which they are
nded are tolerably precise; so that the reference of an
ividual to one or other of them is easy. And when we
e once chosen our class we remain untroubled by any
ther considerations; for since no other statistics are sup-
ed to offer a materially different average, we have no
sion to take account of any other properties than those
ady noticed.

The case of games of chance, already referred to, offers
course an instance of these conditions in an almost ideal
te of perfection; the same circumstances which fit them
eminently for the purposes of fair gambling, fitting them
lly to become examples in Probability. When a die is
e thrown, all persons alike stand on precisely the same
ing of knowledge and of ignorance about the result; the

only data to which any one could appeal being that face turns up on an average once in six times.

§ 5. Let us now examine how far the above condi are fulfilled in the case of problems which discuss wh called the credibility of testimony. The following woul a fair specimen of one of the elementary enquiries ou which these problems are composed;—Here is a staten made by a witness who lies once in ten times, what am conclude about its truth? Objections might fairly be r against the possibility of thus assigning a man his p upon a graduated scale of mendacity. This however we pass over, and will assume that the witness goes about world bearing stamped somehow on his face the approp class to which he belongs, and consequently, the degre credit to which he has a claim on such general grou But there are other and stronger reasons against the missibility of this class of problems.

§ 6. That which has been described in the previ sections as the 'individual' which had to be assigned to appropriate class or series of statistics is, of course, in case, *a statement.* In the particular instance in question individual statement is already assigned to a class, namely of statements made by a witness of a given degre veracity; but it is clearly optional with us whether or not choose to confine our attention to this class in forming judgment; at least it would be optional whenever we practically called on to form an opinion. But in the case this statement, as in that of the mortality of the man wh insurance we were discussing, there are a multitude of o properties observable, besides the one which is supposed mark the given class. Just as in the latter there (besides his age), the place of his birth, the nature of occupation, and so on; so in the former there are (besides eing a statement by a certain kind of witness), the fact of its eing uttered at a certain time and place and under certain ircumstances. At the time the statement is made all these ualities or attributes of the statement are present to us, and e clearly have a right to take into account as many of them s we please. Now the question at present before us seems to e simply this;—Are the considerations, which we might thus ntroduce, as immaterial to the result in the case of the truth f a statement of a witness, as the corresponding considera- ions are in the case of the insurance of a life? There can urely be no hesitation in the reply to such a question. nder ordinary circumstances we soon know all that we can now about the conditions which determine us in judging of he prospect of a man's death, and we therefore rest content ith general statistics of mortality; but no one who heard a itness speak would think of simply appealing to his figure f veracity, even supposing that this had been authoritatively ommunicated to us. The circumstances under which the tatement is made instead of being insignificant, are of over- helming importance. The appearance of the witness, the one of his voice, the fact of his having objects to gain, ogether with a countless multitude of other circumstances hich would gradually come to light as we reflect upon the atter, would make any sensible man discard the assigned verage from his consideration. He would, in fact, no more hink of judging in this way than he would of appealing to he Carlisle or Northampton tables of mortality to determine he probable length of life of a soldier who was already in he midst of a battle.

§ 7. It cannot be replied that under these circumstances e still refer the witness to a class, and judge of his veracity y an average of a more limited kind; that we infer, for ex- mple, that of men who look and act like him under such circumstances, a much larger proportion, say nine-te are found to lie. There is no appeal to a class in this wa all, there is no immediate reference to statistics of any whatever; at least none which we are conscious of usin the time, or to which we should think of resorting for jus cation afterwards. The decision seems to depend upon quickness of the observer's senses and of his apprehen generally.

Statistics about the veracity of witnesses seem in fac be permanently as inappropriate as all other statistics o sionally may be. We may know accurately the percent of recoveries after amputation of the leg; but what surg would think of forming his judgment solely by such ta when he had a case before him? We need not deny course, that the opinion he might form about the patie prospects of recovery might ultimately rest upon the pro tions of deaths and recoveries he might have previously nessed. But if this were the case, these data are lying one may say, obscurely in the background. He does appeal to them directly and immediately in forming judgment. There has been a far more important inter diate process of apprehension and estimation of what essential to the case and what is not. Sharp senses, mem judgment, and practical sagacity have had to be called i play, and there is not therefore the same direct consci and sole appeal to statistics that there was before. surgeon may have in his mind two or three instances which the operation performed was equally severe, but which the patient's constitution was different; the lat element therefore has to be properly allowed for. There be other instances in which the constitution was similar, the operation more severe; and so on. Hence, although ultimate appeal may be to the statistics, it is not so direct heir value has to be estimated through the somewhat hazy nedium of our judgment and memory, which places them nder a very different aspect.

§ 8. Any one who knows anything of the game of whist nay supply an apposite example of the distinction here nsisted on, by recalling to mind the alteration in the nature f our inferences as the game progresses. At the commencenent of the game our sole appeal is rightfully made to the heory of Probability. All the rules upon which each player cts, and therefore upon which he infers that the others will ct, rest upon the observed frequency (or rather upon the requency which calculation assures us will be observed) with which such and such combinations of cards are found to ccur. Why are we told, if we have more than four trumps, o lead them out at once? Because we are convinced, on ure grounds of probability, capable of being stated in the trictest statistical form, that in a majority of instances we hall draw our opponent's trumps, and therefore be left with he command. Similarly with every other rule which is ecognized in the early part of the play.

But as the play progresses all this is changed, and owards its conclusion there is but little reliance upon any ules which either we or others could base upon statistical requency of occurrence, observed or inferred. A multitude f other considerations have come in; we begin to be influenced partly by our knowledge of the character and ractice of our partner and opponents; partly by a rapid ombination of a multitude of judgments, founded upon ur observation of the actual course of play, the grounds f which we could hardly realize or describe at the time nd which may have been forgotten since. That is, the articular combination of cards, now before us, does not eadily fall into any well-marked class to which alone it can reasonably be referred by every one who has the facts him.

§ 9. A criticism somewhat resembling the abo been given by Mill (*Logic*, Bk. III. Chap. xviii. § 3) the applicability of the theory of Probability to the bility of witnesses. But he has added other reasons do not appear to me to be equally valid; he says "co sense would dictate that it is impossible to strike a g average of the veracity, and other qualifications for testimony, of mankind or any class of them; and if it possible, such an average would be no guide, the credibi almost every witness being either below or above the ave The latter objection would however apply with equal to estimating the length of a man's life from tables of tality; for the credibility of different witnesses can sc have a wider range of variation than the length of dif lives. If statistics of credibility could be obtained could be conveniently appealed to when they were obt they might furnish us in the long run with as ac inferences as any other statistics of the same gener scription. These statistics would however in practice rally and rightly be neglected, because there can hard to be circumstances in each individual statement which more appropriately refer it to some new class dependi different statistics, and affording a far better chance being right in that particular case. In most instan the kind in question, indeed, such a change is thus pro in the mode of formation of our opinion, that, as a pointed out, the mental operation ceases to be in any sense founded on appeal to statistics[1].

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[1] It may be remarked also that there is another reason which tends *to* dissuade us from appealing to principles of Probability majority of the cases wher mony has to be estimated. I

.0. The Chance problems which are concerned with tes- y are not altogether confined to such instances as those ·to referred to. Though we must, as it appears to me, all attempts to estimate the credibility of any par- r witness, or to refer him to any assigned class in ɔt of his trustworthiness, and consequently abandon as table any of the numerous problems which start from data as 'a witness who is wrong once in ten times,' t does not follow that testimony may not to a slight .t be treated by our science in a somewhat different ıer. We may be quite unable to estimate, except in 'oughest possible way, the veracity of any particular ›ss, and yet it may be possible to form some kind of on upon the veracity of certain classes of witnesses; y, for instance, that Europeans are superior in this way rientals. So we might attempt to explain why, and to extent, an opinion in which the judgments of ten per- say jurors, concur, is superior to one in which five only ır. Something may also be done towards laying down principles in accordance with which we are to decide her, and why, extraordinary stories deserve less credence ordinary ones, even if we cannot arrive at any precise definite decision upon the point. This last question is .er discussed in the course of the next chapter.

11. The change of view in accordance with which it ws that questions of the kind just mentioned need not ntirely rejected from scientific consideration, presents it-

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ps usually happens, that we .ot absolutely forced to come .ecision; at least so far as the tting of an accused person may nsidered as avoiding a decision. ıy be of much greater import- to us to attain not merely truth on the average, but truth in each individual instance, so that we had rather not form an opinion at all than form one of which we can only say in its justification that it will tend to lead us right in the long run.

self in other directions also. It has, for instance, been alread pointed out that the individual characteristics of any sic man's disease would be quite sufficiently important in mos cases to prevent any surgeon from judging about his recove by a genuine and direct appeal to statistics, however suc considerations might indirectly operate upon his judgmen But if an opinion had to be formed about a considerab number of cases, say in a large hospital, statistics migh again come prominently into play, and be rightly recogniz as the principal source of appeal. We should feel able t compare one hospital, or one method of treatment, wit another. The ground of the difference is obvious. It aris from the fact that the characteristics of the individua which made us so ready to desert the average when we ha to judge of them separately, do not produce the same disturbance when we have to judge about a group of cases The averages then become the most secure and availab ground on which to form an opinion, and therefore Probability again becomes applicable.

But although some resort to Probability may be admitted in such cases as these, it nevertheless does not appear to me that they can ever be regarded as particularly appropriate examples to illustrate the methods and resources of the theory. Indeed it is scarcely possible to resist the conviction that the refinements of mathematical calculation have here been pushed to lengths utterly unjustifiable, when we bear in mind the impossibility of obtaining any corresponding degree of accuracy and precision in the data from which we have to start. To cite but one instance. It would be hard to find a case in which love of consistency has prevailed over common sense to such an extent as in the admission of the conclusion that it is unimportant what are the numbers for and against a particular statement, provided e actual majority is the same. That is, the unanimous dgment of a jury of eight is to count for the same as a ajority of ten to two in a jury of twelve. And yet this nclusion is admitted by Poisson. The assumptions under hich it follows will be indicated in the course of the next hapter.

Again, perfect independence amongst the witnesses or rors is an almost necessary postulate. But where can this e secured? To say nothing of direct collusion, human eings are in almost all instances greatly under the influence f sympathy in forming their opinions. This influence, under he various names of political bias, class prejudice, local eeling, and so on, always exists to a sufficient degree to nduce a cautious person to make many of those individual orrections which we saw to be necessary when we were stimating the trustworthiness, in any given case, of a single witness; that is, they are sufficient to destroy much, if not all, of the confidence with which we resort to statistics and averages in forming our judgment. Since then this Essay is mainly devoted to explaining and establishing the general principles of the science of Probability, we may very fairly be excused from any further treatment of this subject, beyond the brief discussions which are given in the next chapter.

CHAPTER XVII.

ON THE CREDIBILITY OF EXTRAORDINARY STORIES.

§ 1. It is now time to recur for fuller investigation to an enquiry which has been already briefly touched upon more than once; that is, the validity of testimony to establish, as it is frequently expressed, an otherwise improbable story. It will be remembered that in a previous chapter (the twelfth) we devoted some examination to an assertion by Butler, which seemed to be to some extent countenanced by Mill, that a great improbability before the proof might become but a very small improbability after the proof. In opposition to this it was pointed out that the different estimates which we undoubtedly formed of the credibility of the examples adduced, had nothing to do with the fact of the event being past or future, but arose from a very different cause; that the conception of the event which we entertain at the moment (which is all that is then and there actually present to us, and as to the correctness of which as a representation of facts we have to make up our minds) comes before us in two very different ways. In one instance it was a mere guess of our own which we knew from statistics would be right in a certain proportion of cases; in the other instance it was the assertion of a witness, and therefore the appeal was not now primarily to statistics of *the* event, but to the trustworthiness of the witness. The con-tion, or 'event' if we will so term it, had in fact passed t of the category of guesses (on statistical grounds), into at of assertions (most likely resting on some specific evince), and would therefore be naturally regarded in a very fferent light.

§ 2. But it may seem as if this principle would lead us somewhat startling conclusions. For, by transferring the ppeal from the frequency with which the event occurs to he trustworthiness of the witness who makes the assertion, is it not implied that the probability or improbability of an assertion depends solely upon the veracity of the witness? If so, ought not any story whatever to be believed when it is asserted by a truthful person?

In order to settle this question we must look a little more closely into the circumstances under which such testimony is commonly presented to us. As it is of course necessary, for clearness of exposition, to take a numerical example, let us suppose that a given statement is made by a witness who, on the whole and in the long run, is right in what he says nine times out of ten[1]. Here then is an average given to us, an average veracity that is, which includes all the particular statements which the witness has made or will make.

§ 3. Now it has been abundantly shown in a former chapter (Ch. IX. §§ 14—32) that the mere fact of a particular average having been assigned, is no reason for o being forced invariably to adhere to it, even in those ca in which our most natural and appropriate ground of jud ment is found in an appeal to statistics and averag The general average may constantly have to be correct in order to meet more accurately the circumstances of pa ticular cases. In statistics of mortality, for instance, inste of resorting to the wider tables furnished by people general of a given age, we often prefer the narrower tabl furnished by men of a particular profession, abode, or mo of life. The reader may however be conveniently remind here that in so doing we must not suppose that we are abl by any such device, in any special or peculiar way to secu truth. The general average, if persistently adhered t throughout a sufficiently wide and varied experience, woul in the long run tend to give us the truth; all the advantag which the more special averages can secure for us is to giv us the same tendency to the truth with fewer and slighte aberrations.

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[1] Reasons were given in the last chapter against the propriety of applying the rules of Probability with any strictness to such examples as these. But although all approach to numerical accuracy is unattainable, we do undoubtedly recognize in ordinary life a distinction between the credibility of one witness and another; such a rough practical distinction will be quite sufficient for the purposes of this chapter. For convenience, and to illustrate the theory, the examples are best stated in a numerical form, but it is not intended thereby to imply that any such accuracy is really attainable in practice.

§ 4. Returning then to our witness, we know that i we have a very great many statements from him upon al possible subjects, we may feel convinced that in nine out o ten of these he will tell us the truth, and that in the tenth case he will go wrong. This is nothing more than a matter of definition or consistency. But cannot we do better than thus rely upon his general average? Cannot we, in almost any given case, specialize it by attending to various characteristic circumstances in the nature of the statement which he makes; just as we specialize his prospects of mortality by attending to circumstances in his constitution or mode of life?

Undoubtedly we may do this; and in any of the practical contingencies of life, supposing that we were at all guided onsiderations of this nature, we should act very foolishly e did not adopt some such plan. Two methods of thus ecting the average may be suggested: one of them being which practical sagacity would be most likely to employ, other that which is almost universally adopted by writers Probability. The former attempts to make the correction the following considerations: instead of relying upon the ness' general average, we assign to it a sort of conjectural rection to meet the case before us, founded on our expence or observation; that is, we appeal to experience to tablish that stories of such and such a kind are more or s likely to be true, as the case may be, than stories in eral. The other proceeds upon a different and someat more methodical plan. It is here endeavoured to ow, by an analysis of the nature and number of the rces of error in the cases in question, that such and such ds of stories must be more or less likely to be correctly ported, and this in certain numerical proportions.

§ 5. Before proceeding to a discussion of these methods distinction must be pointed out to which writers upon the bject have not always attended, or at any rate to which ey have not generally sufficiently directed their readers' tention[1]. There are, broadly speaking, two different ways which we may suppose testimony to be given. It may, in e first place, take the form of a reply to an alternative estion, a question, that is, framed to be answered by *yes* *no*. Here, of course, the possible answers are mutually ntradictory, so that if one of them is not correct the other st be so:—Has *A* happened, yes or no? The common ode of illustrating this kind of testimony numerically is by supposing a lottery with a prize and blanks, or a bag of of two colours only, the witness knowing that there are two, or at any rate being confined to naming one or oth them. If they are black and white, and he errs when l is drawn, he must say 'white.' The reason for the pı nence assigned to examples of this class is, probably, they correspond to the very important case of verdic juries; juries being supposed to have nothing else to do to say 'guilty' or 'not guilty.'

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[1] I must plead guilty to this rge myself, in the first edition this work. The result was to make the treatment of this part of the subject obscure and imperfect, and in some respects erroneous.

On the other hand, the testimony may take the form more original statement or piece of information. Inste saying, Did *A* happen? we may ask, What happened? if the witness speaks truth he must be supposed, as b to have but one way of doing so; for the occurrence of specific event was of course contemplated. But if he er has many ways of going wrong, possibly an infinite nu Ordinarily however his possible false statements are ass to be limited in number, as must generally be more o the result in practice. This case is represented numeri by supposing the balls in the bag not to be of two co only, but to be all distinct from each other; say by being all numbered successively. It may of course b jected that a large number of the statements that are in the world are not in any way answers to questions, e of the alternative or of the open kind. For instance, a simply asserts that he has drawn the seven of spades fr pack of cards; and we do not know perhaps whether he been asked 'Has that card been drawn?' or 'What car been drawn?' or indeed whether he had been asked any at all. Still more might this be so in the case of any nary historical statement.

This objection is quite to the point, and must be r nized as constituting an additional difficulty. All tha

do is to endeavour, as best we may, to ascertain, from
circumstances of the case, what number of alternatives
witness may be supposed to have had before him. When
imply testifies to some matter well known to be in dis-
, and does not go much into detail, we may fairly con-
that there were practically only the two alternatives
re him of saying 'yes' or 'no.' When, on the other hand,
tells a story of a more original kind, or (what comes to
h the same thing) goes into details, we must regard him
having a wide comparative range of alternatives before
.

These two classes of examples, viz. that of the black and
te balls, in which only one form of error is possible, and
numbered balls, in which there may be many forms of
r, are the only two which we need notice. In practice it
ld seem that they may gradually merge into each other,
rding to the varying ways in which we choose to frame
question. Besides asking, Did you see *A* strike *B*? and,
t did you see? we may introduce any number of inter-
diate leading questions, as, What did *A* do? What did
do to *B*? and so on. In this way we may gradually narrow
possible openings to wrong statement, and so approach
he direct alternative question. But it is clear that all
e cases may be represented numerically by a supposed
inution in the number of the balls which are thus distin-
hed from each other.

§ 6. Of the two plans mentioned in § 4 we will begin
h the latter, as it is the only methodical and scientific one
ch has been proposed. Suppose that there is a bag with
0 balls, only one of which is white, the rest being all
ck. A ball is drawn at random, and our witness whose
acity is $\frac{9}{10}$ reports that the white ball was drawn. Take
eat many of his statements upon this particular subject,

say 10,000; that is, suppose that 10,000 balls having
successively drawn out of this bag, or bags of exactl
same kind, he makes his report in each case. His 1
statements being taken as a fair sample of his genera
rage, we shall find, by supposition, that 9 out of every
them are true and the remaining one false. What w
the nature of these false statements? Under the ci
stances in question, he having only one way of going w
the answer is easy. In the 10,000 drawings the whit
would come out 10 times, and therefore be rightly as
9 times, whilst on the one of these occasions on whi
goes wrong he has nothing to say but 'black.' So wi
9990 occasions on which black is drawn; he is righ
says black on 8991 of them, and is wrong and therefor
white on 999 of them. On the whole, therefore, we co
that out of every 1008 times on which he says that w
drawn he is wrong 999 times and right only 9 times.
is, his special veracity, as we may term it, for cases c
description, has been reduced from $\frac{9}{10}$ to $\frac{9}{1008}$. As it
commonly be expressed, the latter fraction represen
chance that this particular statement of his is true[1].

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[1] The generalized algebraical form of this result is as follows. Let p be the à priori probability of an event, and x be the credibility of the witness. Then, if he asserts that the event happened, the probability that it really did happen is

$$\frac{px}{px+(1-p)(1-x)};$$

whilst if he asserts that it did *not* happen the probability that it did happen is $\frac{p(1-x)}{p(1-x)+(1-p)x}$.

In illustration of some remarks to
be presently made, the rea
notice that on making either
expressions $=p$, we obtain
case $x=\frac{1}{2}$. That is, a witne
veracity $=\frac{1}{2}$ leaves the à prio
ability of an event (of this k
affected.

If, on the other hand, w
these expressions equal to
$1-x$ respectively, we obtain
case $p=\frac{1}{2}$. That is, when a
(of this kind) is as likely to
as not, the ordinary veracit
witness in respect of it rem
affected.

§ 7. We will now take the case in which the witness
many ways of going wrong, instead of merely one. Sup-
e that the balls were all numbered, from 1 to 1,000, and
witness knows this fact. A ball is drawn, and he tells
that it was numbered 25, what are the odds that he is
t? Proceeding as before, in 10,000 drawings this ball
ld be obtained 10 times, and correctly named 9 times.
on the 9990 occasions on which it was not drawn there
ld be a difference, for the witness has now many open-
for error before him. It is, however, generally considered
nable to assume that his errors will all take the form of
uncing wrong numbers; and that, there being no apparent
on why he should choose one number rather than another,
will be likely to announce all the wrong ones equally
n. Hence his 999 errors, instead of all leading him now
k again to one spot, will be uniformly spread over as
y distinct ways of going wrong. On one only of these
sions, therefore, will he mention 25 as having been
wn. It follows therefore that out of every 10 times that
names 25 he is right 9 times; so that in this case his
rage or general truthfulness applies equally well to the
cial case in point.

§ 8. With regard to the truth of these conclusions, it
t of course be admitted that if we grant the validity of
assumptions about the limits within which the blunder-
or mendacity of the witness are confined, and the com-
te impartiality with which his answers are disposed within
se limits, the reasoning is perfectly sound. But are not
se assumptions extremely arbitrary, that is, are not our
teries and bags of balls rendered perfectly precise in many
pects in which, in ordinary life, the conditions supposed to
respond to them are so vague and uncertain that no such
thod of reasoning becomes practically available? Suppose

that a person whom I have long known, and of whos
sure of veracity and judgment I may be supposed th
to have acquired some knowledge, informs me that t
something to my advantage if I choose to go to
trouble or expense in order to secure it. As regar
general veracity of the witness, then, there is no diff
we suppose that this is determined for us. But as r
his story, difficulty and vagueness emerge at every
What is the number of balls in the bag here? What
are the nature and contents of the bag out of which w
pose the drawing to have been made? It does no
that the materials for any rational judgment exis
But if we are to get at any such amended figure of v
as those attained in the above example, these question
necessarily be answered with some degree of accura
the main point of the method consists in determini
often the event must be considered *not* to happen, and
inferring how often the witness will be led wrongly t
that it has happened.

It is not of course denied that considerations of th
in question have some influence upon our decision, b
that this influence could under any ordinary circum
be submitted to numerical determination. We are
less liable to have information given to us that w
come in for some kind of fortune, for instance, w
such good luck has really befallen us; and this n
only but repeatedly. But who can give the faintes
mation of the nature and number of the occasions on
a blank being thus really drawn, a prize will neve
be falsely announced? It appears to me therefor
numerical results of any practical value can seldom,
be looked for from this method of procedure.

§ 9. Our conclusion in the case of the lottery, o mes to the same thing, in the case of the bag with black d white balls, has been questioned or objected to[1] on the ound that it is contrary to all experience to suppose that e testimony of a moderately good witness could be so ormously depreciated under such circumstances. I should efer to base the objection on the ground that experience arcely ever presents such circumstances as those supposed; it if we postulate their existence the given conclusion seems rrect enough. Assume that a man is merely required to y *yes* or *no*; assume also a group or succession of cases in ich *no* should rightly be said very much oftener than . Then, assuming almost any general truthfulness of the ness, we may easily suppose the rightful occasions for ial to be so much the more frequent that a majority of affirmative answers will actually occur as false 'noes' her than as correct 'ayes.' This of course lowers the rage value of his 'ayes,' and renders them comparatively trustworthy.

Consider the following example. I have a gardener whom trust as to all ordinary matters of fact. If he were to l me some morning that my dog had run away I should ly believe him. He tells me however that the dog has e mad. Surely I should accept the statement with much itation, and on the grounds indicated above. It is not t he is more likely to be wrong when the dog *is* mad; t that experience shows that there are other complaints g. fits) which are far more common than madness, and t most of the assertions of madness are erroneous asser- ns referring to these. This seems a somewhat parallel e to that in which we find that most of the assertions t a white ball had been drawn are really false assertions ferring to the drawing of a black ball. Practically I do

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[1] Todhunter's *History*, p. 400. *Philosophical Magazine*, July, 1864.

not think that any one would feel a difficulty in th
orbitantly discounting some particular assertion of a w
whom in most other respects he fully trusted.

§ 10. There is one particular case which has bee
garded as a difficulty in the way of this treatment c
problem, but which seems to me to be a decided con
tion of it; always, be it understood, within the very n
and artificial limits to which we must suppose oursel
be confined. This is the case of a witness whose vera
just one-half; that is, one who, when a mere *yes* or
demanded of him, is as often wrong as right. In the c
any other assigned degree of veracity it is extremely di
to get anything approaching to a confirmation from
tical judgment and experience. We are not accustom
estimate the merits of witnesses in this way, and hardl
preciate what is meant by his numerical degree of tru
ness. But as regards the man whose veracity is one-ha
are (as Mr C. J. Monro has very ingeniously suggested)
too well acquainted with such witnesses, though un
somewhat different name; for this is really nothing else
the case of a person confidently answering a question
a subject-matter of which he knows nothing, and can t
fore only give a mere guess.

Now in the case of the lottery with one prize, whe
witness whose veracity is one-half tells us that we
gained the prize, we find on calculation that his testi
goes for absolutely nothing; the chances that we hav
the prize are just the same as they would be if he had
opened his lips, viz. $\frac{1}{1000}$. But clearly this is what
to be the result, for the witness who knows nothing
the matter leaves it exactly as he found it. He is in
in strictness, scarcely a witness at all; for the natural
tion of a witness is to examine the matter, and so to

nation, more or less, according to his judgment and y, but at any rate to offer an improvement upon the guesser. If, however, we will give heed to his mere we are doing just the same thing as if we were to guess ves, in which case of course the odds that we are right mply measured by the frequency of occurrence of the s.

e cannot quite so readily apply the same rule to the case, namely to that of the numbered balls, for there itness who is right every other time may really be a fair, or even excellent, witness. If he has many ways ing wrong, and yet is right in half his statements, it is that he must have taken some degree of care, and can- ave merely guessed. In a case of *yes* or *no*, any one can ght every other time, but it is different where truth gle and error is manifold. To represent the case of a y worthless witness when there were 1000 balls and rawing of one assigned ball was in question, we should to put his figure of veracity at $\frac{1}{1000}$. If this were we should of course get a similar result.

11. It deserves notice therefore that the figure of ity, or fraction representing the general truthfulness witness, is in a way relative, not absolute; that is, it nds upon, and varies with, the general character of the er which he is supposed to give. Two witnesses of l intrinsic veracity and worth, one of whom confined elf to saying *yes* and *no*, whilst the other ventured to more original assertions, would be represented by rent fractions; the former having set himself a much r task than the latter. The real caution and truthful- of the witness are only one factor, therefore, in his al figure of veracity; the other factor consists of the re of his assertions, as just pointed out. The ordinary

plan therefore, in such problems, of assigning an a truthfulness to the witness, and accepting this alike case of each of the two kinds of answers, though conve seems scarcely sound. This consideration would ho be of much more importance were not the discussions the subject mainly concerned with only one descript answer, namely that of the 'yes or no' kind.

§ 12. So much for the methodical way of treating a problem. The way in which it would be taken in ha those who had made no study of Probability is very diff It would, I apprehend, strike them as follows. They say to themselves, Here is a story related by a witnes tells the truth, say, nine times out of ten. But it is a of a kind which experience shows to be very generally untruly, say 99 times out of 100. Having then these site inducements to belief, they would attempt in som to strike a balance between them. Nothing in the nat a strict rule could be given to enable them to decid they might escape out of the difficulty. Probably, in as they did not judge at haphazard, they would be g by still further resort to experience, or unconscious lections of its previous teachings, in order to settle of the two opposing inductions was better entitled to the day in the particular case before them. The will readily see that any general solution of the pro when thus presented, is impossible. It is simply the familiar case (Chap. IX. §§ 14—32) of an individual belongs equally to two distinct, or even, in respect of characteristics, opposing classes. We cannot decide of to which of the two its characteristics most naturall rightly refer it. A fresh induction is needed in ord settle this point.

§ 13. Rules have indeed been suggested by va riters in order to extricate us from the difficulty. The conroversy about miracles has probably been the most fertile ccasion for suggestions of this kind on one side or the ther. It is to this controversy, presumably, that the phrase s due, so often employed in discussions upon similar subects, 'a contest of opposite improbabilities.' What is meant y such an expression is clearly this: that in forming a udgment upon the truth of certain assertions we may find hat they are comprised in two very distinct classes, so that, ccording as we regarded them as belonging to one or the ther of these distinct classes, our opinion as to their truth ould be very different. Such an assertion belongs to one lass, of course, by its being a statement of a particular itness, or kind of witness; it belongs to the other by its eing a particular kind of story, one of what is called an mprobable nature. Its belonging to the former class is so ar favourable to its truth, its belonging to the latter is so far ostile to its truth. It seems to be assumed, in speaking of contest of opposite improbabilities, that when these different ources of conviction co-exist together, they would each in ome way retain their probative force so as to produce a ontest, ending generally in a victory to one or other of hem. Hume, for instance, speaks of our *deducting* one robability from the other, and apportioning our belief to he remainder[1]. Thomson, in his *Laws of Thought*, speaks f one probability as entirely superseding the other.

§ 14. It does not appear to me that the slightest philosohical value can be attached to any such rules as these. hey doubtless may, and indeed will, hold in individual cases, but they cannot lay claim to any generality. the notion of a contest, as any necessary ingredient in case, must be laid aside. For let us refer again to the in which the perplexity arises, and we shall readily se has just been remarked, that it is nothing more than a ticular exemplification of a difficulty which has already recognized as incapable of solution by any general *à p* method of treatment. All that we are supposed to have b us is a statement. On this occasion it is made by a wi who lies, say, once in ten times in the long run; that is, mostly tells the truth. But on the other hand, it is a s ment which experience, derived from a variety of witness various occasions, assures us is mostly false; stated numeri it is found, let us suppose, to be false 99 times in a hun

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[1] "When therefore these two nds of experience are contrary, we ve nothing to do but subtract the e from the other, and embrace an opinion, either on one side or the other, with that assurance which arises from the remainder." (Essay on Miracles.)

Now, as was shown in the chapter on Induction, w thus brought to a complete dead lock. Our science offe principles by which we can form an opinion, or attem decide the matter one way or the other; for, as we f there are an indefinite number of conclusions which a equally possible. For instance, all the witness' extraord assertions may be true, or they may all be false, or they be divided into the true and the false in any propo whatever. Having gone so far in our appeal to statisti to recognize that the witness is generally right, but tha story is generally false, we cannot stop there. We oug make still further appeal to experience, and ascertain h stands with regard to his stories when they are of particular nature: or rather, for this would be to ma needlessly narrow reference, how it stands with rega stories of that kind when advanced by witnesses o general character, position, sympathies, and so on[1].

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[1] Considerations of this kind have indeed been introduced into the mathematical treatment of th ject. The common algebraica

15. That extraordinary stories are in many cases, pro- in a great majority of cases, less trustworthy than s must be fully admitted. That is, if we were to make distinct classes of such stories respectively, we should hat the same witness, or similar witnesses, were propor- lly more often wrong when asserting the former than asserting the latter. But it does not by any means ar to me that this must always be the case. We may conceive, for instance, that with some people the mere f the story being of a very unusual character may make more careful in what they state, so as actually to add eir veracity. If this were so we might be ready to t their extraordinary stories with even more readiness their ordinary ones.

uch a supposition as that just made does not seem to me y means forced. Put such a case as this: let us sup- that two persons, one of them a man of merely ordinary ty and intelligence, the other a scientific naturalist, a statement about some common event. We believe

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f the problem in § 5 (to begin he simplest case) is of course ows. Let p be the antecedent ility of the event, and t the re of the truthfulness of the s; then the chance of his state- being true is $\frac{pt}{pt+(1-p)(1-t)}$. supposes him to lie as much the event does not happen as it does. But we may meet ses supposed in the text by ing that t' is the measure of racity when the event does appen, so that the above la becomes $\frac{pt}{pt+(1-p)(1-t')}$. Here t' and t measure respectively his trustworthiness in usual and unusual events. As a formal solution this certainly meets the objections stated above in §§ 14 and 15. The determination however of t' would demand, as I have remarked, continually renewed appeal to experience. In any case the practical methods which would be adopted, if any plans of the kind indicated above were resorted to, seem to me to differ very much from that adopted by the mathematicians, in their spirit and plan.

them both. Let them now each report some extraordinary *lusus naturæ* or monstrosity which they profess to have seen. Most persons, we may presume, would receive the statement of the naturalist in this latter case almost as readily as in the former: whereas when the same story came from the unscientific observer it would be received with considerable hesitation. Whence arises the difference? From the conviction that the naturalist will be far more careful, and therefore to the full as accurate, in matters of this kind as in those of the most ordinary description, whereas with the other man we feel by no means the same confidence. Even if any one is not prepared to go this length, he will probably admit that the difference of credit which he would attach to the two kinds of story, respectively, when they came from the naturalist, would be much less than what it would be when they came from the other man.

§ 16. Whilst we are on this part of the subject, it must be pointed out that there is considerable ambiguity and consequent confusion about the use of the term 'an extraordinary story.' Within the province of pure Probability it ought to mean simply a story which asserts an *unusual* event. At least this is the view which has been adopted and maintained, it is hoped consistently, throughout this work. So long as we adhere to this sense we know precisely what we mean by the term. It has a purely objective reference; it simply connotes a very low degree of relative statistical frequency, actual or prospective. Out of a great number of events we suppose a selection of some particular kind to be contemplated, which occurs relatively very seldom, and this is termed an unusual or extraordinary event. It follows, as was abundantly shown in a former chapter, that owing to the rarity of the event we are very little disposed to expect its occurrence in any given case. Our guess about it, in case

thus anticipated it, would very seldom be justified, and
are therefore apt to be much surprised when it does
ur. This, I take it, is the only legitimate sense of 'extra-
linary' so far as Probability is concerned.

But there is another and very different use of the word,
ich belongs to Induction, or rather to the science of
idence in general, more than to that limited portion of it
med Probability. In this sense the 'extraordinary,' and
ll more the 'improbable,' event is not merely one of
treme statistical rarity, which we could not expect to
ess aright, but which on moderate evidence we may pretty
dily accept; it is rather one which possesses, so to say, an
ual evidence-resisting power. It may be something which
ects the credibility of the witness at the fountain-head,
ich makes, that is, his statements upon such a subject
entially inferior to those on other subjects. This is the
e, for instance, with anything which excites his prejudices
passions or superstitions. In these cases it would seem
reasonable to attempt to estimate the credibility of the
tness by calculating (as in § 6) how often his errors would
slead us through his having been wrongly brought to an
irmation instead of adhering correctly to a negation. We
uld rather be disposed to put our correction on the wit-
s' average veracity at once.

§ 17. In true Probability, as has just been remarked,
ry event has its own definitely recognizable degree of
quency of occurrence. It may be excessively rare, rare to
extreme we like to postulate, but still every one who
lerstands and admits the data upon which its occurrence
ends will be able to appreciate within what range of
erience it may be expected to present itself. We do not
ect it in any individual case, nor within any brief range,
we do confidently expect it within an adequately exten-

sive range. How therefore can miraculous stories be
larly taken account of, when the disputants, on one s
least, are not prepared to admit their actual occurrenc
where or at any time? How can any arrangement o
and balls, or other mechanical or numerical illustrati
unlikely events, be admitted as fairly illustrative of m
lous occurrences, or indeed of many of those which
under the designation of 'very extraordinary' or '
improbable'? Those who contest the occurrence of
ticular miracle, as reported by this or that narrator,
admit that miracles are to be confidently expected so
later. It is not a question as to whether what must b
sometimes has happened some particular time, and th
no illustration of the kind can be regarded as apposite.

How unsuitable these merely rare events, howev
cessive their rarity may be, are as examples of mira
events, will be evident from a single consideration. N
I presume, who admitted the occasional occurrence of
ceedingly unusual combination, would be in much d
he considered that he had actually seen it himself[1].
other hand, few men of any really scientific turn
readily accept a miracle even if it appeared to happen
their very eyes. They might be staggered at the tin

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[1] Laplace, for instance (*Essai*, ed. 1825, p. 149), says that if we saw 100 dies (known of course to be fair ones) all give the same face, we should be bewildered at the time, and need confirmation from others, but that, after due examination, no one would feel obliged to postulate hallucination in the matter. But the chance of this occurrence is represented by a fraction whose *numerator* is 1, and denominator contains 77 figures, and is t
utterly inappreciable by th
nation. It must be admitted,
that there is something hyp
about such an example, for w
not really know that the di
fair with a confidence even d
approaching such prodigiou
In other words, it is difficult
keep apart those different as
the question discussed in Ch
§§ 28—33.

ey would probably soon come to discredit it afterwards, so explain it as to evacuate it of all that is meant by iraculous.

§ 18. It appears to me therefore, on the whole, that ry little can be made of these problems of testimony in e way in which it is generally intended that they should treated; that is, in obtaining specific rules for the esti- ation of the testimony under any given circumstances. suming that the veracity of the witness can be measured, e encounter the real difficulty in the utter impossibility of etermining the limits within which the failures of the event question are to be considered to lie, and the degree of plicitness with which the witness is supposed to answer e enquiry addressed to him; both of these being charac- ristics of which it is necessary to have a numerical esti- ate before we can consider ourselves in possession of the quisite data.

Since therefore the practical resource of most persons, z. that of putting a direct and immediate correction, of urse of a somewhat conjectural nature, upon the general ustworthiness of the witness, by a consideration of the ture of the circumstances under which his statement is ade, is essentially unscientific and irreducible to rule; it ally seems to me that there is something to be said in vour of the simple plan of trusting in all cases alike to e witness' general veracity[1]. That is, whether his story ordinary or extraordinary, we may resolve to put it on e same footing of credibility, provided of course that the ent is fully recognized as one which does or may occasionally happen. It is true that we shall thus go constan astray, and may do so to a great extent, so that if th were any rational and precise method of specializing trustworthiness, according to the nature of his story, should be on much firmer ground. But at least we m thus know what to expect on the average. Provided have a sufficient number and variety of statements fr him, and always take them at the same constant rate degree of trustworthiness, we may succeed in balancing correcting our conduct in the long run so as to avoid ruinous error.

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[1] In the first edition this was ted, as it now seems to me, in de- edly too unqualified a manner. must be remembered, however, that (as was shown in § 7) this plan is really the best theoretical one which can be adopted in certain cases.

§ 19. A few words may now be added about the com nation of testimony. No new principles are introduced he though the consequent complication is naturally greater. L us suppose two witnesses, the veracity of each being Now suppose 100 statements made by the pair; according the plan of proceeding adopted before, we should have th both right 81 times and both wrong once, in the remaini 18 cases one being right and the other wrong. But sin they are both supposed to give the same account, what w have to compare together are the number of occasions o which they agree and are right, and the total number on whi they agree whether right or wrong. The ratio of the form to the latter is the fraction which expresses the trustworth ness of their combination of testimony in the case in questio

In attempting to decide this point the only difficulty i in determining how often they will be found to agree whe they are both wrong, for clearly they must agree when the are both right. This enquiry turns of course upon the num ber of ways in which they can succeed in going wron Suppose first the case of a simple *yes* or *no* (as in § 6), an take the same example, of a bag with 1000 balls, in whic *one only* is white. Proceeding as before, we should find tha of 100,000 drawings (the number required in order to ain a complete cycle of all possible occurrences, as well as ll possible reports about them) the two witnesses agree a correct report of the appearance of white in 81, and ee in a wrong report of it in 999. The Probability there- of the story when so attested is $\frac{81}{1080}$; the fact therefore two such witnesses of equal veracity having concurred kes the report nearly 9 times as likely as when it rested n the authority of only one of them[1].

§ 20. When however the witnesses have many ways of ng wrong, the fact of their agreeing makes the report far e likely to be true. For instance, in the case of the 1000 nbered balls, it is very unlikely that when they both mis-e the number they should (without collusion) happen to ke the same misstatement. Whereas, in the last case, ry combined misstatement necessarily led them both to assertion that the event in question had happened, we uld now find that only once in 999 × 999 times would y both be led to assert that *some given number* (say, as re, 25) had been drawn. The odds in favour of the

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[1] It is on this principle that the rkable conclusion mentioned on 05 is based. Suppose an event se probability is p; and that, of mber of witnesses of the same city (y), m assert that it hap-d, and n deny this. Generaliz-he arithmetical reasoning given e we see that the chance of the t being asserted varies as

$$^{m}(1-y)^n+(1-p)\,y^n(1-y)^m;$$

as the chance that the event ens, and that m are right and n rong; *plus* the chance that it not happen, and that n are right and m are wrong). And the chance of its being rightly asserted as $py^m(1-y)^n$. Therefore the chance that when we have an assertion before us it is a true one is

$$\frac{py^m(1-y)^n}{py^m(1-y)^n+(1-p)\,y^n(1-y)^m},$$

which is equal to

$$\frac{py^{m-n}}{py^{m-n}+(1-p)\,(1-y)^{m-n}}.$$

But this last expression represents the probability of an assertion which is unanimously supported by $m-n$ such witnesses.

event in fact now become $\frac{80919}{80920}$, which are enorm greater than when there was only one witness.

It appears therefore that when two, and of course more when many, witnesses agree in a statement in a m about which they might make many and various erro combination of their favourable testimony adds enorm to the likelihood of the event; provided always that is no chance of collusion. And in the extreme case o opportunities for error being, as they well may be, practi infinite in number, such combination would produce a perfect certainty. But then this condition, viz. absen collusion, very seldom can be secured. Practically our source of error and suspicion is in the possible existen some kind of collusion. Since we can seldom entirely rid of this danger, and when it exists it can never be mitted to numerical calculation, it appears to me that co nation of testimony, in regard to detailed accounts, is more unfitted for consideration in Probability than even of single testimony.

§ 21. The impossibility of any adequate or even ap priate consideration of the credibility of miraculous st by the rules of Probability has been already noticed in § But, since the grounds of this impossibility are often insufficiently appreciated, a few pages may conveniently added here with a view to enforcing this point. If it regarded as a digression, the importance of the subject the persistency with which various writers have at one or another attempted to treat it by the rules of our sci must be the excuse for entering upon it.

A necessary preliminary will be to decide upon some d nition of a miracle. It will, we may suppose, be admitted most persons that in calling a miracle 'a suspension of a *of* causation,' we are giving what, though it may not am

an adequate definition, is at least true as a description.
is true, though it may not be the whole truth. Whatever
e the miracle may be, this is its physical aspect: this is the
int at which it comes into contact with the subject-matter
science. If it were not considered that any suspension of
sation were involved, the event would be regarded merely
an ordinary one to which some special significance was
ached, that is, as a type or symbol rather than a miracle.
is this aspect moreover of the miracle which is now ex-
sed to the main brunt of the attack, and in support of
ich therefore the defence has generally been carried on.

Now it is obvious that this, like most other definitions or
scriptions, makes some assumption as to matters of fact,
d involves something of a theory. The assumption clearly
that laws of causation prevail universally, or almost uni-
rsally, throughout nature, so that infractions of them are
arked and exceptional. This assumption is made, but it
es not appear that anything more than this is necessarily
quired; that is, there is nothing which need necessarily
ake us side with either of the two principal schools which
e divided as to the nature of these laws of causation. The
finition will serve equally well whether we understand by
w nothing more than uniformity of antecedent and conse-
uent, or whether we assert that there is some deeper and
ore mysterious tie between the events than mere sequence.
he use of the term 'causation' in this minimum of signifi-
tion is common to both schools, though the one might
nsider it inadequate; we may speak, therefore, of 'suspen-
ons of causation' without committing ourselves to either.

§ 22. It should be observed that the aspect of the ques-
n suggested by this definition is one from which we can
rdly escape. Attempts indeed have been sometimes made
avoid the necessity of any assumption as to the universal

prevalence of law and order in nature, by defining a m from a different point of view. A miracle may be calle instance, 'an immediate exertion of creative power,' 'a of a revelation,' or, still more vaguely, an 'extraord event.' But nothing would be gained by adopting any definitions as these. However they might satisfy the t logian, the student of physical science would not rest co with them for a moment. He would at once assert his belief, and that of other scientific men, in the existen universal law, and enquire what was the connection o definition with this doctrine. An answer would imperati be demanded to the question, Does the miracle, as you described it, imply an infraction of one of these laws, or it not? And an answer must be given, unless indee reject his assumption by denying our belief in the exist of this universal law, in which case of course we put selves out of the pale of argument with him. The nece of having to recognize this fact is growing upon men da day, with the increased study of physical science. And s this aspect of the question has to be met some time or o it is as well to place it in the front. The difficulty, i scientific form, is of course a modern one, for the doctrine of which it arises is modern. But it is only one instance, of many that might be mentioned, in which the growt some philosophical conception has gradually affected nature of the dispute, and at last shifted the position of battle-ground, in some discussion with which it might nc first have appeared to have any connection whatever.

§ 23. So far our path is plain. Up to this point disci of very different schools may advance together; for in la down the above doctrine we have carefully abstained f implying or admitting that it contains the whole truth. *from* this point two paths branch out before us, path ferent from each other in their character, origin, and rection, as can well be conceived. As this enquiry is only digression, we may confine ourselves to stating briefly what em to be the characteristics of each, without attempting to ve the arguments which might be used in their support.

(I.) On the one hand, we may assume that this principle causation is the ultimate one. By so terming it, we do ot mean that it is one from which we consciously start in ur investigations, as we do from the axioms of geometry, ut rather that it is the final result towards which we find urselves drawn by a study of nature. Finding that, hroughout the scope of our enquiries, event follows event in ever-failing uniformity, and finding moreover (some might ld) that this experience is supported or even demanded by tendency or law of our nature (it does not matter here how e describe it), we may come to regard this as the one ndamental principle on which all our enquiries should rest.

(II.) Or, on the other hand, we may admit a class of inciples of a very different kind. Allowing that there is is uniformity so far as our experience extends, we may yet dmit what can hardly be otherwise described than by lling it a Superintending Providence, that is, a Scheme or rder, in reference to which Design may be predicated ithout using merely metaphorical language. To adopt an ptly chosen distinction, it is not to be understood as *over-uling* events, but rather as *underlying* them.

§ 24. Now it is quite clear that according as we come to e discussion of any particular miracle or extraordinary ory under one or other of these prepossessions, the question of its credibility will assume a very different aspect. It sometimes overlooked that although a difference about *cts* is one of the conditions of a *bonâ fide* argument, a difference which reaches to ultimate principles is fatal to all

argument. The possibility of present conflict is banishe
such a case as absolutely as that of future concord. A l
amount of popular literature on the subject of mira
seems to labour under this defect. Arguments are sta
and examined for and against the credibility of miracul
stories without the disputants appearing to have
adequate conception of the chasm which separates one
from the other.

§ 25. The following illustration may serve in so
degree to show the sort of inconsistency of which we
speaking. A sailor reports that in some remote coral isl
of the Pacific, on which he had landed by himself, he
found a number of stones on the beach disposed in the e
form of a cross. Now if we conceive a debate to arise ab
the truth of his story, in which it is attempted to decide
matter simply by considerations about the validity of te
mony, without introducing the question of the existenc
inhabitants, and the nature of their customs, we shall h
some notion of the unsatisfactory nature of many of
current arguments about miracles. All illustrations of
subject are imperfect, but a case like this, in which a
posed trace of human agency is detected interfering with
orderly sequence of other and non-intelligent natural ca
is as much to the point as any illustration can be.
thing omitted here from the discussion is clearly the one
portant thing. If we suppose that there is no inhabitant
shall probably disbelieve the story, or consider it t
grossly exaggerated. If we suppose that there are inha
ants, the question is at once resolved into a different
somewhat more intricate one. The credibility of the wit
is not the only element, but we should necessarily hav
take into consideration the character of the supposed in
itants, and the object of such an action on their part.

26. Considerations of this character are doubtless introduced into the discussion, but it appears to me they are introduced to a very inadequate extent. It is urged, after Paley, 'Once believe in a God, and miraare not incredible.' Such an admission surely demands e modification and extension. It should rather be stated , Believe in a God whose working may be traced throughthe whole moral and physical world. It amounts, in to this;—Admit that there may be a *design* which we trace somehow or other in the course of things; admit we are not wholly confined to tracing the connection of its, or following out their effects, but that we can form e idea, feeble and imperfect though it be, of a *scheme*[1]. y's advice sounds too much like saying, Admit that there fairies, and we can account for our cups being cracked. admission is not to be made in so off-hand a manner. any one labouring under the difficulty we are speaking his belief in a God almost out of any constant relation to ure, whom we then imagine to occasionally manifest himin a perhaps irregular manner, is altogether impossible. only form under which belief in the Deity can gain ence into his mind is as the controlling Spirit of an infinite orderly system. In fact, it appears to me, paradoxical he suggestion may appear, that it might even be more for a person thoroughly imbued with the spirit of Inive science, though an atheist, to believe in a miracle ch formed a part of a vast system, than for such a person, theist, to accept an isolated miracle.

§ 27. It is therefore with great prudence that Hume, others after him, have practically insisted on commencing a discussion of the credibility of the single miracle, treating the question as though the Christian Revel could be adequately regarded as a succession of such e As well might one consider the living body to be repres by the aggregate of the limbs which compose it. What be complained of in so many popular discussions on the ject is the entire absence of any recognition of the dif ground on which the attackers and defenders of miracl so often really standing. Proofs and illustrations are duced in endless number, which involving, as they almo do in the mind of the disputants on one side at least, very principle of causation, the absence of which in the in question they are intended to establish, they fail i single essential point. To attempt to induce any one t believe in the existence of physical causation, in a instance, by means of illustrations which to him seem additional examples of the principle in question, is lik ing to make a dam, in order to stop the flow of a riv shovelling in snow. Such illustrations are plentiful in of controversy, but being in reality only modified for that which they are applied to counteract, they change shape at their first contact with the disbeliever's mind only help to swell the flood which they were intend check.

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[1] he stress which Butler lays upon this notion of a scheme is, I think, reat merit of his *Analogy*.

CHAPTER XVIII.

HE NATURE AND USE OF AN AVERAGE, AND ON THE DIFFERENT KINDS OF AVERAGE[1].

We have had such frequent occasion to refer to es, and to the kind of uniformity which they are apt lay in contrast with individual objects or events, that now be convenient to discuss somewhat more minutely re the different kinds of available average, and what are the functions they perform.

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e is much need of some good accessible to the ordinary reader, of the nature and s of the principal kinds of The common text-books of suggest that there are only h, viz. the arithmetical, the cal and the harmonical:— luding two with which the an has little or nothing to excluding two or more with should have a great deal to e best three references I can reader are the following. article *Moyenne* in the *Dic-* *des Sciences Médicales*, by llon. This is written some- om the Quetelet point of 2) A paper by Fechner in the *Abhandlungen d. Math. phys. Classe d. Kön. Sächs. Gesellschaft d. Wiss.* 1878; pp. 1—76. This contains a very interesting discussion, especially for the statistician, of a number of different kinds of mean. His account of the median is remarkably full and valuable. But little mathematical knowledge is demanded. (3) A paper by Mr F. Y. Edgeworth in the *Camb. Phil. Trans.* for 1885, entitled *Observations and Statistics.* This demands some mathematical knowledge. Instead of dealing, as such investigations generally do, with only one Law of Error and with only one kind of mean, it covers a wide field of investigation.

The first vague notion of an average, as we now u stand it, seems to me to involve little more than tha something *intermediate* to a number of objects. The o must of course resemble each other in certain respects, wise we should not think of classing them together they must also differ in certain respects, otherwise we s not distinguish between them. What the average do us, under this primitive form, is to enable us convenien retain the group together as a whole. That is, it furni sort of representative value of the quantitative aspect things in question, which will serve for certain purpo take the place of any single member of the group.

It would seem then that the first dawn of the conce which science reduces to accuracy under the designat an average or mean, and then proceeds to subdivide various distinct species of means, presents itself as forming some of the functions of a general name. For is the main use of a general name? It is to reduce larity of objects to unity; to group a number of t together by reference to some qualities which they p in common. The ordinary general name rests upon a siderable variety of attributes, mostly of a quali character, whereas the average, in so far as it serve same sort of purpose, rests rather upon a single quanti attribute. It directs attention to a certain kind and d of magnitude. When the grazier says of his sheep that with another they will fetch about 50 shillings,' o farmer buys a lot of poles which 'run to about 10 feet,' true that they are not strictly using the equivalent of a general or a collective name. But they are coming near to such use, in picking out a sort of type or specim the magnitude to which attention is to be directed, an *classing* the whole group by its resemblance to this

The grazier is thinking of his sheep: not in a merely general sense, as sheep, and therefore under that name or conception, but as sheep of a certain approximate money value. Some will be more, some less, but they are all near enough to the assigned value to be conveniently classed together as if by a name. Many of our rough quantitative designations seem to be of this kind, as when we speak of 'eight-day clocks' or 'twelve-stone men,' &c.; unless of course we intend (as we sometimes do in these cases) to assign a maximum or minimum value. It is not indeed easy to see how else we could readily convey a merely general notion of the quantitative aspect of things, except by selecting a type as above, or by assigning certain limits within which the things are supposed to lie.

§ 2. So far there is not necessarily any idea introduced of comparison,—of comparison, that is, of one group with another,—by aid of such an average. As soon as we begin to think of this we have to be more precise in saying what we mean by an average. We can easily see that the number of possible kinds of average, in the sense of intermediate values, is very great; is, in fact, indefinitely great. Out of the general conception of an intermediate value, obtained by some treatment of the original magnitudes, we can elicit as many subdivisions as we please, by various modes of treatment. There are however only three or four which for our purposes need be taken into account.

(1) In the first place there is the arithmetical average or mean. The rule for obtaining this is very simple: add all the magnitudes together, and divide the sum by their number. This is the only kind of average with which the unscientific mind is thoroughly familiar. But we must not let this simplicity and familiarity blind us to the fact that there are definite reasons for the employment of this average,

and that it is therefore appropriate only in definite ci stances. The reason why it affords a safe and acc intermediate value for the actual divergent values, is for many of the ordinary purposes of life, such as pur and sale, we come to exactly the same result, wheth take account of those existent divergences, or suppo the objects equated to their average. What the g must be understood to mean, if he wishes to be accura saying that the average price of his sheep is 50 shillin that so far as that flock is concerned (and so far as concerned), it comes to exactly the same thing, whether are each sold at different prices, or are all sold at the ' age' price. Accordingly, when he compares his sales o year with those of another; when he says that last yea sheep averaged 48 shillings against the 50 of this year employment of this representative or average value is a simplification, and is perfectly accurate for the purpo question.

§ 3. (2) Now consider this case. A certain populat found to have doubled itself in 100 years: can we talk 'average' increase here of 1 per cent. annually? The cumstances are not quite the same as in the former case the analogy is sufficiently close for our purpose. The a is decidedly, No. If 100 articles of any kind are sold for we say that the average price is £1. By this we mean the total amount is the same whether the entire lot are for £100, or whether we split the lot up into indivi and sell each of these for £1. The average price here convenient fictitious substitute, which can be applied each individual without altering the aggregate total. therefore the question be, Will a supposed increase of 1 in each of the 100 years be equivalent to a total increas *double* the original amount? we are proposing a clo

alogous question. And the answer, as just remarked, must
in the negative. An annual increase of 1 p. c. continued
r 100 years will more than double the total; it will multiply
by about 2·7. The true annual increment required is mea-
ured by $\sqrt[100]{2}$; that is, the population may be said to have
creased 'on the average' 0·7 p. c. annually.

We are thus directed to the second kind of average dis-
ssed in the ordinary text-books of algebra, viz. the geome-
ical. When only two quantities are concerned, with a single
termediate value between them, the geometrical mean con-
ituting this last is best described as the mean proportional
tween the two former. Thus, since $3 : \sqrt{15} :: \sqrt{15} : 5$,
15 is the geometrical mean between 3 and 5. When a
umber of geometrical means have to be interposed between
wo quantities, they are to be so chosen that every term in
e entire succession shall bear the same constant ratio to
s predecessor. Thus, in the example in the last paragraph,
9 intermediate steps were to be interposed between 1 and 2,
ith the condition that the 100 ratios thus produced were to
e all equal.

It would seem therefore that wherever accurate quantita-
ve results are concerned, the selection of the appropriate
ind of average must depend upon the answer to the ques-
on, What particular intermediate value may be safely
bstituted for the actual variety of values, so far as the
recise object in view is concerned? This is an aspect of
e subject which will have to be more fully considered in
e next chapter. But it may safely be laid down that for
urposes of general comparison, where accurate numerical
lations are not required, almost any kind of intermediate
alue will answer our purpose, provided we adhere to the
me throughout. Thus, if we want to compare the statures
f the inhabitants of different counties or districts in Eng-

land, or of Englishmen generally with those of Frenc or to ascertain whether the stature of some particula or district is increasing or diminishing, it really do seem to matter what sort of average we select provi course, that we adhere to the same throughout our i gations. A very large amount of the work perform averages is of this merely comparative or non-quant description; or, at any rate, nothing more than this is required. This being so, we should naturally resort arithmetical average; partly because, having been l the field, it is universally understood and appealed t partly because it happens to be remarkably simple an to calculate.

§ 4. The arithmetical mean is for most ordinar poses the simplest and best. Indeed, when we are d with a small number of somewhat artificially selected tudes, it is the only mean which any one would th employing. We should not, for instance, apply any method to the results of a few dozen measurements of l or estimates of prices.

When, however, we come to consider the results of large number of measurements of the kind which grouped together into some sort of 'probability cur begin to find that there is more than one alternative us. Begin by recurring to the familiar curve repre on p. 29; or, better still, to the initial form of it repre in the next chapter (p. 476). We see that there ar different ways in which we may describe the vertex curve. We may call it the position of the *maximun* nate; or that of the *centre* of the curve; or (as will b hereafter) the point to which the arithmetical aver all the different values of the variable magnitude dire These three are all distinct ways of describing a po

en we are dealing with a symmetrical curve at all
ling the binomial or exponential form they all three
e in giving the same result: as they obviously do in
e in question.

soon, however, as we come to consider the case of
etrical, or lop-sided curves, the indications given by
hree methods will be as a rule quite distinct; and
re the two former of these deserve brief notice as
nting different kinds of means from the arithmetical
nary one. We shall see that there is something about
them which recommends it to common sense as being
e way natural and appropriate.

(3) The first of these selects from amongst the
different magnitudes that particular one which is
requently represented. It has not acquired any tech-
esignation[1], except in so far as it is referred to, by
phical representation, as the "maximum ordinate"
l. But I suspect that some appeal to such a mean
ndard is really far from uncommon, and that if we
draw out into clearness the conceptions latent in the
nts of the comparatively uncultivated, we should find
ere were various classes of cases in which this mean
turally employed. Suppose, for instance, that there
fishery in which the fish varied very much in size

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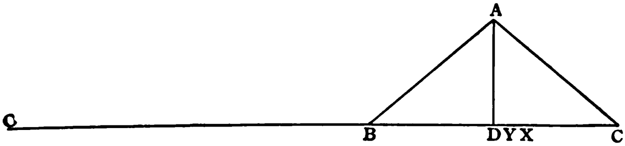
kind of mean is called by
and others the "*dichteste*
The most appropriate ap-
t that I have seen is by Prof.
assenerscheinungen, p. 42)
e shows that it indicates
sort of normal length of
life, of about 70 years; a
ich is almost entirely mask-
we appeal to the arithmetical average.

This mean *ought* to be called the 'probable' value (a name however in possession of another) on the ground that it indicates the point of likeliest occurrence; i.e. if we compare all the indefinitely small and equal units of variation, the one corresponding to this will tend to be most frequently represented.

but in which the commonest size was somewhat near t largest or the smallest. If the men were in the habit selling their fish by *weight*, it is probable that they wou before long begin to acquire some kind of notion of wh is meant by the arithmetical mean or average, and wou perceive that this was the most appropriate test. But if t fish were sorted into sizes, and sold by numbers in each these sizes, I suspect that this appeal to a maximum ord nate would begin to take the place of the other. That i the most numerous class would come to be selected as sort of type by which to compare the same fishery at o time and another, or one fishery with others. There is al as we shall see in the next chapter, some scientific grou for the preference of this kind of mean in peculiar case viz. where the quantities with which we deal are tr 'errors,' in the estimate of some magnitude, and where al it is of much more importance to be exactly right, or ve nearly right, than to have merely a low average of error.

§ 6. (4) The remaining kind of mean is that which now coming to be called the "median." It is one wi which the writings of Mr Galton have done so much familiarize statisticians, and is best described as follow Conceive all the objects in question to be marshalled in t order of their magnitude; or, what comes to the same thin conceive them sorted into a number of equally numero classes; then the middle one of the row, or the middle o in the middle class, will be the *median*. I do not thin that this kind of mean is at all generally recognized present, but if Mr Galton's scheme of natural measureme by what he calls "per-centiles" should come to be gene ally adopted, such a test would become an important on There are some conspicuous advantages about this kind o mean. For one thing, in most statistical enquiries, it i the simplest to calculate; and, what is more, the process determining it serves also to assign another important ment to be presently noticed, viz. the 'probable error.' en again, as Fechner notes, whereas in the arithmetical an a few exceptional and extreme values will often cause plexity by their comparative preponderance, in the case the median (where their number only and not their exme magnitude is taken into account) the importance of ch disturbance is diminished.

§ 7. A simple illustration will serve to indicate how these ree kinds of mean coalesce into one when we are dealing th symmetrical Laws of Error, but become quite distinct soon as we come to consider those which are unsymetrical.



ppose that, in measuring a magnitude along *OBDC*, here the extreme limits are *OB* and *OC*, the law of error represented by the triangle *BAC*: the length *OD* will at once the arithmetical mean, the median, and the most equent length: its frequency being represented by the aximum ordinate *AD*. But now suppose, on the other nd, that the extreme lengths are *OD* and *OC*, and that e triangle *ADC* represents the law of error. The most equent length will be the same as before, *OD*, marked by e maximum ordinate *AD*. But the mean value will now OX, where $DX = \frac{1}{3}DC$; and the median will be OY, here $DY = \left(1 - \frac{1}{\sqrt{2}}\right)DC$.

Another example, taken from natural phenomena be found in the heights of the barometer as taken same hour on successive days. So far as 4857 of thes be regarded as furnishing a sufficiently stable basis perience, it certainly seems that the resulting curve quency is asymmetrical. The mean height here was to be 29·98: the median was 30·01: the most fre height was 30·05. The close approximation amongst is an indication that the asymmetry is slight[1].

§ 8. It must be clearly understood that the aver whatever kind it may be, from the mere fact of its b single substitute for an actual plurality of observed must let slip a considerable amount of information. I it is only introduced for economy. It may entail n when used for some one assigned purpose, as in o ample about the sheep; but for purposes in gene cannot possibly take the place of the original diversi yielding all the information which they contained. this is to be retained we must resort to some other m Practically we generally do one of two things: either put all the figures down in statistical tables, or (2) v peal to a diagram. This last plan is convenient wh data are very numerous, or when we wish to display discover the nature of the law of facility under whicl range.

The mere assignment of an average lets drop nea of this, confining itself to the indication of an interm

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[1] A diagram illustrative of this number of results was given in *Nature* (Sept. 1, 1887). In calculating, as above, the different means, I may remark that the original results were given to three decimal places; but, in classing them, only one place was noted. That is, 29·9 i all values between 29·900 and Thus the value most freque tered in my tables was 30·0, the usual principles of inter this is reckoned as 30·05.

ne. It gives a "middle point" of some kind, but says
hing whatever as to how the original magnitudes were
uped about this point. For instance, whether two mag-
udes had been respectively 25 and 27, or 15 and 37, they
uld yield the same arithmetical average of 26.

§ 9. To break off at this stage would clearly be to leave
problem in a very imperfect condition. We therefore
urally seek for some simple test which shall indicate
closely the separate results were grouped about their
rage, so as to recover some part of the information which
been let slip.

If any one were approaching this problem entirely anew,—
t is, if he had no knowledge of the mathematical exi-
cies which attend the theory of "Least Squares,"—I ap-
hend that there is but one way in which he would set
out the business. He would say, The average which we
e already obtained gave us a rough indication, by as-
ning an intermediate point amongst the original magni-
des. If we want to supplement this by a rough indica-
n as to how near together these magnitudes lie, the
t way will be to treat their departures from the mean
hat are technically called the "errors") in precisely the
ne way, viz. by assigning *their* average. Suppose there
13 men whose heights vary by equal differences from
feet to 6 feet, we should say that their average height
s 66 inches, and their average departure from this average
s $3\frac{3}{13}$ inches.

Looked at from this point of view we should then pro-
d to try how each of the above-named averages would
wer the purpose. Two of them,—viz. the arithmetical
n and the median,—will answer perfectly; and, as we
ll immediately see, are frequently used for the purpose.
too we could, if we pleased, employ the geometrical

mean, though such employment would be tedious, ow to the difficulty of calculation. The 'maximum ordi clearly would not answer, since it would generally (v. diagram on p. 443) refer us back again to the ave already obtained, and therefore give no information.

The only point here about which any doubt could a concerns what is called in algebra the *sign* of the er Two equal and opposite errors, added algebraically, w cancel each other. But when, as here, we are regard the errors as substantive quantities, to be considered their own account, we attend only to their real magnit and then these equal and opposite errors are to be put o exactly the same footing.

§ 10. Of the various means already discussed, two just remarked, are in common use. One of these is miliarly known, in astronomical and other calculations the 'Mean Error,' and is so absolutely an application of same principle of the arithmetical mean to the errors, t has been already applied to the original magnitudes, th needs no further explanation. Thus in the example in last section the mean of the heights was 66 inches, mean of the errors was $3\frac{3}{13}$ inches.

The other is the Median, though here it is always kn under another name, i.e. as the 'Probable Error';—a t nical and decidedly misleading term. It is briefly def as that error which we are as likely to exceed as to short of: otherwise phrased, if we were to arrange all errors in the order of their magnitude, it corresponds to t one of them which just bisects the row. It is therefore 'median' error: or, if we arrange all the magnitudes in s cessive order, and divide them into four equally numer classes,—what Mr Galton calls 'quartiles,'—the first a third of the consequent divisions will mark the limits e 'probable error' on each side, whilst the middle one will ark the 'median.' This median, as was remarked, coincides, symmetrical curves, with the arithmetical mean.

It is best to stand by accepted nomenclature, but the eader must understand that such an error is not in any rict sense 'probable.' It is indeed highly improbable that any particular instance we should happen to get just this ror: in fact, if we chose to be precise and to regard it as e exact magnitude out of an infinite number, it would be finitely unlikely that we should hit upon it. Nor can it be id to be probable that we shall be *within* this limit of the uth, for, by definition, we are just as likely to exceed as to ll short. As already remarked (see note on p. 441), the maximum ordinate' would have the best right to be regarded s indicating the really most probable value.

§ 11. (5) *The error of mean square.* As previously ggested, the plan which would naturally be adopted by y one who had no concern with the higher mathematics of he subject, would be to take the 'mean error' for the purpose of the indication in view. But a very different kind of verage is generally adopted in practice to serve as a test of the amount of divergence or dispersion. Suppose that we have the magnitudes $x_1, x_2, \dots x_n$; their ordinary average is $\frac{1}{n}(x_1 + x_2 + . + x_n)$, and their 'errors' are the differences between this and $x_1, x_2, \dots x_n$. Call these errors $e_1, e_2, \dots e_n$, then the arithmetical mean of these errors (irrespective of sign) is $\frac{1}{n}(e_1 + e_2 + \dots + e_n)$. The Error of Mean Square[1], on the other hand, is the square root of $\frac{1}{n}(e_1^2 + e_2^2 + \dots + e_n^2)$.

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[1] There is some ambiguity in the phraseology in use here. Thus Airy commonly uses the expression 'Error of Mean Square' to represent, as

The reasons for employing this latter kind of aver preference to any of the others will be indicated in th lowing chapter. At present we are concerned only wit general logical nature of an average, and it is the sufficient to point out that any such intermediate valu answer the purpose of giving a rough and summary i tion of the degree of closeness of approximation whic various measures display to each other and to their co average. If we were to speak respectively of the 'first the 'second average,' we might say that the former of assigns a rough single substitute for the plurality of or values, whilst the latter gives a similar rough estimate degree of their departure from the former.

§ 12. So far we have only been considering the g nature of an average, and the principal kinds of a practically in use. We must now enquire more partic what are the principal purposes for which averages ar ployed.

In this respect the first thing we have to do is to doubts in the reader's mind on a subject on whi perhaps has not hitherto felt the slightest doubt. one is more or lest familiar with the practice of appeal an average in order to secure accuracy. But distinctly we begin by doing is to sacrifice accuracy; for in pl the plurality of actual results we get a single result

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here, $\sqrt{\frac{\Sigma e^2}{n}}$. Galloway commonly speaks of the 'Mean Square of the Errors' to represent $\frac{\Sigma e^2}{n}$. I shall adhere to the former usage and represent it briefly by E. M. S. Still more unfortunate (to my thinking) *is the* employment, by Mr Merriman and others, of the expression Error,' (widely in use in it natural signification,) as the lent of this E. M. S.

The technical term 'Fluct is applied by Mr F. Y. Edgew the expression $\frac{2\Sigma e^2}{n}$.

possibly does not agree with any one of them. If I find temperature in different parts of a room to be different, say that the average temperature is 61°, there may per- be but few parts of the room where this exact tempera- is realized. And if I say that the average stature of rtain small group of men is 68 inches, it is probable that ne of them will present precisely this height.

The principal way in which accuracy can be thus secured hen what we are really aiming at is not the magnitudes re us but something else of which they are an indication. hey are themselves 'inaccurate,'—we shall see presently this needs some explanation,—then the single average, h in itself agrees perhaps with none of them, may be h more nearly what we are actually in want of. We shall it convenient to subdivide this view of the subject into parts; by considering first those cases in which quantita- considerations enter but slightly, and in which no deter- ation of the particular Law of Error involved is demanded, secondly those in which such determination cannot be ded. The latter are only noticed in passing here, as a rate chapter is reserved for their fuller consideration.

§ 13. The process, as a practical one, is familiar enough lmost everybody who has to work with measures of any l. Suppose, for instance, that I am measuring any object a brass rod which, as we know, expands and contracts rding to the temperature. The results will vary slightly, g sometimes a little too great and sometimes a little too ll. All these variations are physical facts, and if what were concerned with was the properties of brass they ld be the one important fact for us. But when we are erned with the length of the object measured, these facts ome superfluous and misleading. What we want to do is scape their influence, and this we are enabled to effect by taking their (arithmetical) average, provided only th as often in excess as in defect[1]. For this purpose all t necessary is that equal excesses and defects shou equally prevalent. It is not necessary to know what law of variation, or even to be assured that it is of on ticular kind. Provided only that it is in the langu the diagram on p. 29, symmetrical, then the arithn average of a suitable and suitably varied number of me ments will be free from this source of disturbance. what holds good of this cause of variation will hold g all others which obey the same general conditions. I the equal prevalence of equal and opposite errors see be the sole and sufficient justification of the familiar p of taking the average in order to secure accuracy.

§ 14. We must now make the distinction to whi tention requires so often to be drawn in these su between the cases in which there respectively is, and some objective magnitude aimed at: a distinction whi common use of the same word "errors" is so apt to ol When we talked, in the case of the brass rod, of ex and defects being equal, we meant exactly what we sa that for every case in which the 'true' length (i.e. th termined by the authorized standard) is exceeded by a fraction of an inch, there will be a corresponding c which there is an equal defect.

On the other hand, when there is no such fixed obj standard of reference, it would appear that all that we by equal excesses and defects is permanent symme arrangement. In the case of the measuring rod we

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[1] Practically, of course, we should allow for the expansion or contraction. But for purposes of logical explanation we may conveniently take this variation as a speci one of those disturbances whi be neutralised by resort to a rage.

le to start with something which existed, so to say, before
variations; but in many cases any starting point which
can find is solely determined by the average.

Suppose, for instance, we take a great number of ob-
rvations of the height of the barometer at a certain place,
all times and seasons and in all weathers, we should
erally consider that the average of all these showed the
rue' height for that place. What we really mean is that
e height at any moment is determined partly (and prin-
pally) by the height of the column of air above it, but partly
o by a number of other agencies such as local temperature,
oisture, wind, &c. These are sometimes more and some-
mes less effective, but their range being tolerably con-
ant, and their distribution through this range being
lerably symmetrical, the average of one large batch of
servations will be almost exactly the same as that of any
her. This constancy of the average *is* its truth. I am
ite aware that we find it difficult not to suppose that
ere must be something more than this constancy, but we
e probably apt to be misled by the analogy of the other
ss of cases, viz. those in which we are really aiming at
me sort of mark.

§ 15. As regards the practical methods available for
termining the various kinds of average there is very little
o be said; as the arithmetical rules are simple and definite,
d involve nothing more than the inevitable drudgery
tendant upon dealing with long rows of figures. Perhaps
he most important contribution to this part of the subject is
rnished by Mr Galton's suggestion to substitute the median
r the mean, and thus to elicit the average with sufficient
curacy by the mere act of grouping a number of objects
gether. Thus he has given an ingenious suggestion for
taining the average height of a number of men without

the trouble and risk of measuring them all. "A barb chief might often be induced to marshall his men in order of their heights, or in that of the popular estimat their skill in any capacity; but it would require some paratus and a great deal of time to measure each separately, even supposing it possible to overcome the us strong repugnance of uncivilized people to any such ceeding" (*Phil. Mag.* Jan. 1875). That is, it being kn from wide experience that the heights of any toler homogeneous set of men are apt to group themselves s metrically,—the condition for the coincidence of the t principal kinds of mean,—the middle man of a row arranged in order will represent the mean or average i and him we may subject to measurement. Moreover, s the intermediate heights are much more thickly represe than the extreme ones, a moderate error in the selecti the central man of a long row will only entail a very s error in the selection of the corresponding height.

§ 16. We can now conveniently recur to a subject w has been already noticed in a former chapter, viz. the tempt which is sometimes made to establish a distin between an average and a mean. It has been propose confine the former term to the cases in which we are dea with a fictitious result of our own construction, that is, a mere arithmetical deduction from the observed ma tudes, and to apply the latter to cases in which the supposed to be some objective magnitude peculiarly re sentative of the average.

Recur to the three principal classes, of things approp to Probability, which were sketched out in Ch. II. § 4. first of these comprised the results of games of chance. a die ten times: the total number of pips on the u side may vary from ten up to sixty. Suppose it to

irty. We then say that the average of this batch of
n is three. Take another set of ten throws, and we may
t another average, say four. There is clearly nothing
jective peculiarly corresponding in any way to these
erages. No doubt if we go on long enough we shall
d that the averages tend to centre about 3·5: we then
ll this *the* average, or the 'probable' number of points;
d this ultimate average might have been pretty con-
ntly asserted beforehand from our knowledge of the con-
itution of a die. It has however no other truth or reality
out it of the nature of a type: it is simply the limit
wards which the averages tend.

The next class is that occupied by the members of most
tural groups of objects, especially as regards the charac-
istics of natural species. Somewhat similar remarks may
repeated here. There is very frequently a 'limit' towards
hich the averages of increasing numbers of individuals tend
approach; and there is certainly some temptation to re-
rd this limit as being a sort of type which all had been
ended to resemble as closely as possible. But when we
ked closer, we found that this view could scarcely be
tified; all which could be safely asserted was that this
e represented, for the time being, the most numerous
ecimens, or those which under existing conditions could
st easily be produced.

The remaining class stands on a somewhat different
ound. When we make a succession of more or less suc-
sful attempts of any kind, we get a corresponding series
deviations from the mark at which we aimed. These we
y treat arithmetically, and obtain their averages, just as
the former cases. These averages are fictions, that is to
they are artificial deductions of our own which need
necessarily have anything objective corresponding to

them. In fact, if they be averages of a *few* only most probably will not have anything thus correspon to them. Anything answering to a type can only be so in the 'limit' towards which they ultimately tend, for limit coincides with the fixed point or object aimed at.

§ 17. Fully admitting the great value and intere Quetelet's work in this direction,—he was certainly the to direct public attention to the fact that so many class natural objects display the same characteristic property, nevertheless does not seem desirable to attempt to m such a distinction by any special use of these tech terms. The objections are principally the two following.

In the first place, a single antithesis, like this betw an average and a mean, appears to suggest a very m simpler state of things than is actually found to exis nature. A reference to the three classes of things mentioned, and a consideration of the wide range and versity included in each of them, will serve to remin not only of the very gradual and insensible advance what is thus regarded as 'fictitious' to what is claime 'real;' but also of the important fact that whereas the ' type' may be of a fluctuating and evanescent character, 'fiction' may (as in games of chance) be apparently f for ever. Provided only that the conditions of produc remain stable, averages of large numbers will always p tically present much the same general characteristics. far more important distinction lies between the averag a few, with its fluctuating values and very imperfect occasional attainment of its ultimate goal, and the ave of many and its gradually close approximation to its mate value: i.e. to its objective point of aim if there hap to be such.

Then, again, the considerations adduced in this cha ill show that within the field of the average itself there is r more variety than Quetelet seems to have recognized. e did not indeed quite ignore this variety, but he prac- ically confined himself almost entirely to those symmetrical rangements in which three of the principal means coalesce to one. We should find it difficult to carry out his dis- ction in less simple cases. For instance, when there is me degree of asymmetry, it is the 'maximum ordinate' hich would have to be considered as a 'mean' to the clusion of the others; for no appeal to an arithmetical erage would guide us to this point, which however is to e regarded, if any can be so regarded, as marking out the sition of the ultimate type.

§ 18. We have several times pointed out that it is a aracteristic of the things with which Probability is con- rned to present, in the long run, a continually intensifying iformity. And this has been frequently described as what ppens 'on the average.' Now an objection may very ssibly be raised against regarding an arrangement of ings by virtue of which order thus emerges out of disorder deserving any special notice, on the ground that from the ture of the arithmetical average it could not possibly be therwise. The process by which an average is obtained, it ay be urged, insures this tendency to equalization amongst e magnitudes with which it deals. For instance, let there a party of ten men, of whom four are tall and four are hort, and take the average of any five of them. Since this umber cannot be made up of tall men only, or of short men ly, it stands to reason that the averages cannot differ so uch amongst themselves as the single measures can. Is ot then the equalizing process, it may be asked, which is bservable on increasing the range of our observations, e which can be shown to follow from necessary laws of arithmetic, and one therefore which might be asserted priori?

Whatever force there may be in the above objection ari principally from the limitations of the example selected, which the number chosen was so large a proportion of total as to exclude the bare possibility of only extreme ca being contained within it. As much confusion is often f here between what is necessary and what is matter of perience, it will be well to look at an example somew more closely, in order to determine exactly what are really necessary consequences of the averaging process.

§ 19. Suppose then that we take ten digits at rand from a table (say) of logarithms. Unless in the highly likely case of our having happened upon the same digit times running, the average of the ten *must* be intermedi between the possible extremes. Every conception of average of any sort not merely involves, but actually mea the taking of something intermediate between the extrem The average therefore of the ten must lie closer to 4·5 (t average of the extremes) than did some of the single digits

Now suppose we take 1000 such digits instead of 10. W can say nothing more about the larger number, with d monstrative certainty, than we could before about the small If they were unequal to begin with (i.e. if they were not the same) then the average *must* be intermediate, but mo than this cannot be proved arithmetically. By comparison wi such purely arithmetical considerations there is what may called a physical fact underlying our confidence in the gro ing stability of the average of the larger number. It is th the constituent elements from which the average is deduc will themselves betray a growing uniformity:—that the pr portions in which the different digits come out will becom more and more nearly equal as we take larger numbers hem. If the proportions in which the 1000 digits were istributed were the same as those of the 10 the averages ould be the same. It is obvious therefore that the arithmetical process of obtaining an average goes a very little ay towards securing the striking kind of uniformity which e find to be actually presented.

§ 20. There is another way in which the same thing ay be put. It is sometimes said that whatever may have een the arrangement of the original elements the process of ontinual averaging will necessarily produce the peculiar inomial or exponential law of arrangement. This statement is perfectly true (with certain safeguards) but it is not n any way opposed to what has been said above. Let us ake for consideration the example above referred to. The rrangement of the individual digits in the long run is the implest possible. It would be represented, in a diagram, ot by a curve but by a finite straight line, for each digit ccurs about as often as any other, and this exhausts all the 'arrangement' that can be detected. Now, when we consider the results of taking averages of ten such digits, we see t once that there is an opening for a more extensive arrangement. The totals may range from 0 up to 100, and therefore the average will have 100 values from 0 to 9; and what e find is that the frequency of these numbers is determined ccording to the Binomial[1] or Exponential Law. The most requent result is the true mean, viz. 4·5, and from this they iminish in each direction towards 0 and 10, which will each ccur but once (on the average) in 10^{10} occasions.

The explanation here is of the same kind as in the former ase. The resultant arrangement, so far as the averages are

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[1] More strictly *multinomial:* the elative frequency of the different umbers being indicated by the coefficients of the powers of x in the development of

$$(1+x+x^2+\ldots+x^9)^{10}.$$

concerned, is only ‘necessary’ in the sense that it is a ı
sary result of certain physical assumptions or experi
If all the digits tend to occur with equal frequency, a
they are ‘independent’ (i.e. if each is associated indiffe
with every other), then it is an arithmetical conseq
that the averages when arranged in respect of their m
tude and prevalence will display the Law of Facility
indicated. Experience, so far as it can be appealed to,
that the true randomness of the selection of the digits,
their equally frequent recurrence, and the impartiali
their combination,—is very fairly secured in practice.
cordingly the theoretic deduction that whatever may
been the original Law of Facility of the individual r
we shall always find the familiar Exponential Law ass
itself as the law of the averages, is fairly justified b
perience in such a case.

The further discussion of certain corrections and r
ments is reserved to the following chapter.

§ 21. In regard to the three kinds of average emp
to test the amount of dispersion,—i.e. the mean erro
probable error, and the error of mean square,—tw
portant considerations must be borne in mind. The
both recur for fuller discussion and justification in the
of the next chapter, when we come to touch upon the M
of Least Squares, but their significance for logical pu
is so great that they ought not to be entirely passed
present.

(1) In the first place, then, it must be remarked t
order to know what in any case is the real value of an
we ought in strictness to know what is the position
limit or ultimate average, for the amount of an er
always theoretically measured from this point. But t
information which we do not always possess. Recu

ce more to the three principal classes of events with which e are concerned, we can readily see that in the case of mes of chance we mostly do possess this knowledge. Inead of appealing to experience to ascertain the limit, we actically deduce it by simple mechanical or arithmetical nsiderations, and then the 'error' in any individual case or oup of cases is obviously found by comparing the results us obtained with that which theory informs us would ultiately be obtained in the long run. In the case of deberate efforts at an aim (the third class) we may or may t know accurately the value or position of this aim. In tronomical observations we do not know it, and the method Least Squares is a method for helping us to ascertain it as ll as we can; in such experimental results as firing at a ark we do know it, and may thus test the nature and ount of our failure by direct experience. In the remaing case, namely that of what we have termed natural kinds groups of things, not only do we not know the ultimate mit, but its existence is always at least doubtful, and in any cases may be confidently denied. Where it does exist, at is, where the type seems for all practical purposes peranently fixed, we can only ascertain it by a laborious resort statistics. Having done this, we may then test by it the ults of observations on a small scale. For instance, if we d that the ultimate proportion of male to female births is bout 106 to 100, we may then compare the statistics of me particular district or town and speak of the consequent rror,' viz. the departure, in that particular and special strict, from the general average.

What we have therefore to do in the vast majority of actical cases is to take the average of a finite number of easurements or observations,—of all those, in fact, which e have in hand,—and take *this* as our starting point in order to measure the errors. The errors in fact are known for certain but only probably calculated. This h ever is not so much of a theoretic defect as it may seen first sight; for inasmuch as we seldom have to employ t methods,—for purposes of calculation, that is, as distinguis from mere illustration,—except for the purpose of covering what the ultimate average is, it would be a so petitio principii to assume that we had already secure But it is worth while considering whether it is desirabl employ one and the same term for 'errors' known to such, and whose amount can be assigned with certainty, for 'errors' which are only probably such and whose am can be only probably assigned. In fact it has been propo to employ the two terms 'error' and 'residual' respecti to distinguish between the magnitudes thus determined, is, between the (generally unknown) actual error and the served error.

§ 22. (2) The other point involves the question to w extent either of the first two tests (pp. 446, 7) of the c ness with which the various results have grouped themse about their average is trustworthy or complete. The ans is that they are necessarily incomplete. No single estin or magnitude can possibly give us an adequate account number of various magnitudes. The point is a very portant one; and is not, I think, sufficiently attended to, consequence being, as we shall see hereafter, that it is too summarily assumed that a method which yields result with the least 'error of mean square' must necessa be the best result for all purposes. It is not however by means clear that a test which answers best for one pur must do so for all.

It must be clearly understood that each of these test

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[1] By Mr Merriman, in his work on *Least Squares*.

n 'average,' and that every average necessarily rejects a ass of varied detail by substituting for it a single result. e had, say, a lot of statures: so many of 60 inches, so ny of 61, &c. We replace these by an 'average' of 68, d thereby drop a mass of information. A portion of this e then seek to recover by reconsidering the 'errors' or partures of these statures from their average. As before, wever, instead of giving the full details we substitute an erage of the errors. The only difference is that instead of king the same kind of average (i.e. the arithmetical) we ten prefer to adopt the one called the 'error of mean uare.'

§ 23. A question may be raised here which is of sufficient portance to deserve a short consideration. When we have t a set of measurements before us, why is it generally ld to be sufficient simply to assign: (1) the mean value; d (2) the mean departure from this mean? The answer , of course, partly given by the fact that we are only supsed to be in want of a rough approximation: but there is re to be said than this. A further justification is to be nd in the fact that we assume that we need only conmplate the possibility of a single Law of Error, or at any e that the departures from the familiar Law will be but fling. In other words, if we recur to the figure on p. 29, e assume that there are only two unknown quantities or posable constants to be assigned; viz. first, the position of e centre, and, secondly, the degree of eccentricity, if one ay so term it, of the curve. The determination of the ean value directly and at once assigns the former, and the termination of the mean error (in either of the ways rered to already) indirectly assigns the latter by confining us one alone of the possible curves indicated in the figure.

Except for the assumption of one such Law of Error the

determination of the mean error would give but a
intimation of the sort of outline of our Curve of Fa
We might then have found it convenient to adopt some
of successive approximation, by adding a third or
'mean.' Just as we assign the mean value of the m
tude, and its mean departure from this mean; so we
take this mean error (however determined) as a fresh st
point, and assign the mean departure from it. If the
were worth further discussion we might easily illustra
means of a diagram the sort of successive approxim
which such indications would yield as to the ultimate
of the Curve of Facility or Law of Error.

As this volume is written mainly for those who take an interest
logical questions involved, rather than as an introduction to the
processes of calculation, mathematical details have been throughout a
as much as possible. For this reason comparatively few reference
been made to the exponential equation of the Law of Error, or
corresponding 'Probability integral,' tables of which are given in
handbooks on the subject. There are two points however in con
with these particular topics as to which difficulties are, or should be,
so many students that some notice may be taken of them here

(1) In regard to the ordinary algebraical expression for the law of
viz. $y = \frac{h}{\sqrt{\pi}} e^{-h^2x^2}$, it will have been observed that I have always spoke
as being *proportional* to the number of errors of the particular magnit
It would hardly be correct to say, absolutely, that y *represents* that nu
because of course the actual number of errors of any precise magn
where continuity of possibility is assumed, must be indefinitely smal
therefore we want to pass from the continuous to the discrete, by ascert
the actual number of errors between two consecutive divisions of our
when, as usual in measurements, all within certain limits are refer
some one precise point, we must modify our formula. In accordance
the usual differential notation, we must say that the number of errors f
into one subdivision (dx) of our scale *is* $dx \frac{h}{\sqrt{\pi}} e^{-h^2x^2}$, where dx is a (
unit of length, in which both h^{-1} and x must be measured.

difficulty felt by most students is in applying the formula to actual :s, in other words in putting in the correct units. To take an actual :al example, suppose that 1460 men have been measured in regard to eight "true to the nearest inch," and let it be known that the :s here is 3·6 inches. Then $dx = 1$ (inch); $h^{-1} = 3{\cdot}6$ inches. Now

$e^{-h^2x^2}\,dx = 1$; that is, the sum of all the consecutive possible values

l to *unity*. When therefore we want the sum, as here, to be 1460, we xpress the formula thus;—$y = \frac{1460}{\sqrt{\pi} \times 3{\cdot}6} e^{-\left(\frac{x}{3{\cdot}6}\right)^2}$, or $y = 228\, e^{-\left(\frac{x}{3{\cdot}6}\right)^2}$.

stands for the number of inches measured from the central or mean and y stands for the number of men referred to that height in our cal table. (The values of e^{-t^2} for successive values of t are given in ıdbooks.)

illustration I give the calculated numbers by this formula for values ›m 0 to 8 inches, with the actual numbers observed in the Cambridge ·ements recently set on foot by Mr Galton.

inches	calculated	observed
$x=0$	$y=228$	$=231$
$x=1$	$y=212$	$=218$
$x=2$	$y=166$	$=170$
$x=3$	$y=111$	$=110$
$x=4$	$y=\ 82$	$=\ 66$
$x=5$	$y=\ 32$	$=\ 31$
$x=6$	$y=\ 11$	$=\ 10$
$x=7$	$y=\ \ 4$	$=\ \ 6$
$x=8$	$y=\ \ 1$	$=\ \ 3$

·e the average height was 69 inches: dx, as stated, $=1$ inch. By , 'put $x = 0$,' we mean, calculate the number of men who are assigned .nches; i.e. who fall between 68·5 and 69·5. By saying, 'put $x = 4$,' ın, calculate the number who are assigned to 65 or to 73; i.e. who lie ı 64·5 and 65·5, or between 72·5 and 73·5. The observed results, it seen, keep pretty close to the calculated: in the case of the former *ıns* of equal and opposite divergences from the mean have been taken, ual results not being always the same in opposite directions.

The other point concerns the interpretation of the familiar pro-

7 integral, $\frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2}dt$. Every one who has calculated the chance

event, by the help of the tables of this integral given in so many ›oks, knows that if we assign any numerical value to t, the ›onding value of the above expression assigns the chance that an error taken at random shall lie within that same limit, viz. t. Thu $t=1{\cdot}5$, and we have the result ·96; that is, only 4 *per cent.* of the erro exceed 'one and a half.' But when we ask, 'one and a half' *what?* answer would not always be very ready. As usual, the main difficul the beginner is not to manipulate the formulæ, but to be quite clear a his units.

It will be seen at once that this case differs from the preceding in we cannot now choose our unit as we please. Where, as here, there is one variable (t), if we were allowed to select our own unit, the inch, foo whatever it might be, we might get quite different results. Accordi some comparatively natural unit must have been chosen for us in whic are bound to reckon, just as in the circular measurement of an angl distinguished from that by degrees.

The answer is that the unit here is the *modulus*, and that to put '$t=$ is to say, 'suppose the error half as great again as the modulus'; modulus itself being an error of a certain assignable magnitude depen upon the nature of the measurements or observations in question. We see this better if we put the integral in the form $\frac{2}{\sqrt{\pi}}\int_0^{hx} e^{-h^2x^2}\,d(hx)$; whi precisely equivalent, since the value of a definite integral is independe the particular variable employed. Here hx is the same as $x : \frac{1}{h}$; i.e. the ratio of x to $\frac{1}{h}$, or x measured in terms of $\frac{1}{h}$. But $\frac{1}{h}$ is the modulus i equation $\left(y=\frac{h}{\sqrt{\pi}}e^{-h^2x^2}\right)$ for the law of error. In other words the merical value of an error in this formula, is the number of times, whol fractional, which it contains the modulus.

CHAPTER XIX.

THE THEORY OF THE AVERAGE AS A MEANS OF APPROXIMATION TO THE TRUTH.

1. IN the last chapter we were occupied with the Average ainly under its qualitative rather than its quantitative pect. That is, we discussed its general nature, its principal rieties, and the main uses to which it could be put in dinary life or in reasoning processes which did not claim to e very exact. It is now time to enter more minutely into e specific question of the employment of the average in e way peculiarly appropriate to Probability. That is, we ust be supposed to have a certain number of measureents,—in the widest sense of that term,—placed before us, d to be prepared to answer such questions as; Why do we ke their average? With what degree of confidence? ust we in all cases take the average, and, if so, one always f the same kind?

The subject upon which we are thus entering is one hich, under its most general theoretic treatment, has perhaps given rise to more profound investigation, to a greater ariety of opinion, and in consequence to a more extensive istory and literature, than any other single problem within he range of mathematics[1]. But, in spite of this, the main logical principles underlying the methods and process question are not, I apprehend, particularly difficult to g though, owing to the extremely technical style of treat adopted even in comparatively elementary discussions o subject, it is far from easy for those who have but a mod command of mathematical resources to disentangle t principles from the symbols in which they are clothed. present chapter contains an attempt to remove these diff ties, so far as a general comprehension of the subject is cerned. As the treatment thus adopted involves a siderable number of subdivisions, the reader will prob find it convenient to refer back occasionally to the tabl contents at the commencement of this volume.

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[1] Mr Mansfield Merriman pubished in 1877 (*Trans. of the Connecticut Acad.*) a list of 408 writings on the subject of Least Squares.

§ 2. The subject, in the form in which we shall dis it, will be narrowed to the consideration of the *averag* account of the comparative simplicity and very wide p lence of this aspect of the problem. The problem is how very commonly referred to, even in non-mathematical treat as the Rule or Method of Least Squares; the fact b that, in such cases as we shall be concerned with, the of Least Squares resolves itself into the simpler and familiar process of taking the arithmetical average. A simple example,—one given by Herschel,—will explain general nature of the task under a slightly wider treatn and will serve to justify the familiar designation.

Suppose that a man had been firing for some time w pistol at a small mark, say a wafer on a wall. We may it for granted that the shot-marks would tend to g themselves about the wafer as a centre, with a density v ing in some way inversely with the distance from the ce But now suppose that the wafer which marked the ce was removed, so that we could see nothing but the surfa the wall spotted with the shot-marks; and that we

guess the position of the wafer. Had there been
shot, common sense would suggest our assuming
e very precariously) that this marked the real centre.
re been two, common sense would suggest our taking
-point between them. But if three or more were
, common sense would be at a loss. It would feel
ne intermediate point ought to be selected, but
ot see its way to a more precise determination, be-
familiar reliance,—the arithmetical average,—does
at hand here. The rule in question tells us how to
It directs us to select that point which will render
of the squares of all the distances of the various
ks from it the least possible[1].

is merely by way of illustration, and to justify the
designation of the rule. The sort of cases with
e shall be exclusively occupied are those compara-
mple ones in which only linear magnitude, or some
which can be adequately represented by linear mag-
s the object under consideration. In respect of these
e of Least Squares reduces itself to the process of
he average, in the most familiar sense of that term,
arithmetical mean; and a single Law of Error, or its
l equivalent, a Curve of Facility, will suffice accu-
indicate the comparative frequency of the different
of the one variable magnitude involved.

ıer words, we are to take
re of gravity" of the shot-
arding them all as of equal
his is, in reality, the 'aver-
the marks, as the elemen-
ıetrical construction for
the centre of gravity of a
points will show; but it is
arly so regarded. Of course,
when we are dealing with such cases as occur in Mensuration, where we have to combine or reconcile three or more inconsistent equations, some such rule as that of Least Squares becomes imperative. No taking of an average will get us out of the difficulty.

§ 3. We may conveniently here again call attention misconception or confusion which has been already no in a former chapter. It is that of confounding the La Error with the Method of Least Squares. These are tl of an entirely distinct kind. The former is of the natu a physical fact, and its production is one which in n cases is entirely beyond our control. The latter,—or simplified application of it, such as the arithmetical averag is no law whatever in the physical sense. It is rath precept or rule for our guidance. The Law states, in given case, how the errors tend to occur in respect of t magnitude and frequency. The Method directs us ho treat these errors when any number of them are prese to us. No doubt there is a relation between the two, as be pointed out in the course of the following pages; there is nothing really to prevent us from using the s method for different laws of error, or different method the same law. In so doing, the question of distinct r and wrong would seldom be involved, but rather one of r or less propriety.

§ 4. The reader must understand,—as was implie the illustration about the pistol shots,—that the ulti problem before us is an *inverse* one. That is, we are posed to have a moderate number of 'errors' before us we are to undertake to say whereabouts is the centre f which they diverge. This resembles the determination cause from the observation of an effect. But, as m happens in inverse problems, we must commence with consideration of the direct problem. In other words, so as concerns the case before us, we shall have to begin supposing that the ultimate object of our aim,—that is, true centre of our curve of frequency,—is already known us: in which case all that remains to be done is to study nsequences of taking averages of the magnitudes which nstitute the errors.

§ 5. We shall, for the present, confine our remarks to hat must be regarded as the typical case where conderations of Probability are concerned; viz. that in which e law of arrangement or development is of the Binomial ind. The nature of this law was explained in Chap. II., here it was shown that the frequency of the respective umbers of occurrences was regulated in accordance with he magnitude of the successive terms of the expansion of he binomial $(1+1)^n$. It was also pointed out that when n ecomes very great, that is, when the number of influencing ircumstances is very large, and their relative individual fluence correspondingly small, the form assumed by a rve drawn through the summits of ordinates representing ese successive terms of the binomial tends towards that signed by the equation

$$y = Ae^{-h^2x^2}.$$

For all practical purposes therefore we may talk inifferently of the Binomial or Exponential law; if only on e ground that the arrangement of the actual phenomena one or other of these two schemes would soon become distinguishable · when the numbers involved are large. ut there is another ground than this. Even when the henomena themselves represent a continuous magnitude, r measurements of them,—which are all with which we n deal,—are discontinuous. Suppose we had before us the curate heights of a million adult men. For all practical rposes these would represent the variations of a connuous magnitude, for the differences between two sucssive magnitudes, especially near the mean, would be appreciably small. But our tables will probably represent them only to the nearest inch. We have so many assig as 69 inches; so many as 70; and so on. The tab statement in fact is of much the same character as if were assigning the number of 'heads' in a toss of a han of pence; that is, as if we were dealing with discontin numbers on the binomial, rather than with a contin magnitude on the exponential arrangement.

§ 6. Confining ourselves then, for the present, to general head, of the binomial or exponential law, we n distinguish two separate cases in respect of the knowle we may possess as to the generating circumstances of variable magnitudes.

(1) There is, first, the case in which the condition the problem are determinable à priori: that is, where we able to say, prior to specific experience, how frequently e combination will occur in the long run. In this case main or ultimate object for which we are supposing that average is employed,—i.e. that of discovering the true m value,—is superseded. We are able to say what the m or central value in the long run will be; and therefore th is no occasion to set about determining it, with some tro and uncertainty, from a small number of observations. S it is necessary to discuss this case carefully, because assumption is a necessary link in the reasoning in ot cases.

This comparatively à priori knowledge may present it in two different degrees as respects its completeness. In first place it may, so far as the circumstances in ques are concerned, be absolutely complete. Consider the res when a handful of ten pence is repeatedly tossed up. know precisely what the mean value is here, viz. eq division of heads and tails: we know also the chance of heads and four tails, and so on. That is, if we had to t a diagram showing the relative frequency of each com- ation, we could do so without appealing to experience. could draw the appropriate binomial curve from the erating conditions given in the statement of the problem. But now consider the results of firing at a target con- ing of a long and narrow strip, of which one point is rked as the centre of aim[1]. Here (assuming that there no causes at work to produce permanent bias) we know t this centre will correspond to the mean value. And we w also, in a general way, that the dispersion on each side this will follow *a* binomial law. But if we attempted to t out the proportions, as in the preceding case, by erecting inates which should represent each degree of frequency ve receded further from the mean, we should find that could not do so. Fresh data must be given or inferred. ood marksman and a bad marksman will both distribute r shot according to the same general law; but the dity with which the shots thin off as we recede from the re will be different in the two cases. Another 'constant' manded before the curve of frequency could be correctly ed out.

§ 7. (2) The second division, to be next considered, esponds for all logical purposes to the first. It com- s the cases in which though we have no à priori know- e as to the situation about which the values will tend to ter in the long run, yet we have sufficient experience at to assign it with practical certainty. Consider for nce the tables of human stature. These are often very nsive, including tens or hundreds of thousands. In such s the mean or central value is determinable with just as great certainty as by any à priori rule. That is, if w another hundred thousand measurements from the class of population, we should feel secure that the a would not be altered by any magnitude which our mea instruments could practically appreciate.

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[1] The only reason for supposing xceptional shape is to secure sim- y. The ordinary target, allowing errors in two dimensions, would yield slightly more complicated results.

§ 8. But the mere assignment of the mean or value does not here, any more than in the precedin give us all that we want to know. It *might* so happe the mean height of two populations was the same, bı the law of dispersion about that mean was very dif so that a man who in one series was an exceptional g dwarf should, in the other, be in no wise remarkable.

To explain the process of thus determining the magnitude of the dispersion would demand too much ı matical detail; but some indication may be given. we have to do is to determine the constant h in the eqı $y=\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$. In technical language, what we have tı to determine the *modulus* of this equation. The quaı in the above expression is called the modulus. It me the degree of contraction or dispersion about the indicated by this equation. When it is large the disp is considerable; that is the magnitudes are not

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[1] When first referred to, the *general* form of this equation was given (v. p. 29). The special form here assigned, in which $\frac{h}{\sqrt{\pi}}$ is substituted for A, is commonly employed in Probability, because the integral of ydx, between $+\infty$ and $-\infty$, becomes equal to unity. That is, the sum of all the mutually *exclusive* possibilities is represented, as usual, by unity. In this expression h is a quantity order x^{-1}; for hx is to be a nu quantity, standing as it doe index. The modulus, being t procal of this, is of the sam of quantities as the errors then In fact, if we multiply it by we have the so-called 'p error.'

owded up towards the centre, when it is small they are ss crowded up. The smaller the modulus in the curve presenting the thickness with which the shot-marks stered about the centre of the target, the better the rksman.

§ 9. There are several ways of determining the modulus. the first of the cases discussed above, where our theo- tical knowledge is complete, we are able to calculate it à iori from our knowledge of the chances. We should turally adopt this plan if we were tossing up a large ndful of pence.

The usual à posteriori plan, when we have the measure- ents of the magnitudes or observations before us, is this:— ke the mean square of the errors, and double this; the re- lt gives the square of the modulus. Suppose, for instance, at we had the five magnitudes, 4, 5, 6, 7, 8. The mean of ese is 6: the 'errors' are respectively 2, 1, 0, 1, 2. There- re the 'modulus squared' is equal to $\frac{10}{5}$; i.e. the modulus is $\sqrt{2}$. Had the magnitudes been 2, 4, 6, 8, 10; representing e same mean (6) as before, but displaying a greater disper- on about it, the modulus would have been larger, viz. $\sqrt{8}$ stead of $\sqrt{2}$.

Mr Galton's method is more of a graphical nature. It described in a paper on Statistics by Intercomparison *hil. Mag.* 1875), and elsewhere. It may be indicated as llows. Suppose that we were dealing with a large number measurements of human stature, and conceive that all the rsons in question were marshalled in the order of their ight. Select the average height, as marked by the central an of the row. Suppose him to be 69 inches. Then raise r depress) the scale from this point until it stands at such a height as just to include one half of the men above below) the mean. (In practice this would be found to quire about 1·71 inches: that is, one quarter of any l group of such men will fall between 69 and 70·71 inc Divide this number by ·4769 and we have the modulus. the case in question it would be equal to about 3·6 inche

Under the assumption with which we start, viz. that law of error displays itself in the familiar binomial form in some form approximating to this, the three methods i cated above will coincide in their result. Where ther any doubt on this head, or where we do not feel able to culate beforehand what will be the rate of dispersion must adopt the second plan of determining the mod This is the only universally applicable mode of calcula in fact that it should yield the modulus is a truth of de tion; for in determining the error of mean square we really doing nothing else than determining the modulu was pointed out in the last chapter.

§ 10. The position then which we have now reache this. Taking it for granted that the Law of Error wil into the symbolic form expressed by the equation $y = \frac{h}{\sqrt{\pi}} e$ we have rules at hand by which h may be determined. therefore, for the purposes in question, know all about curve of frequency: we can trace it out on paper: g one value,—say the central one,—we can determine other value at any distance from this. That is, kno how many men in a million, say, are 69 inches high, we determine without direct observation how many will be 68, 70, 71, and so on.

We can now adequately discuss the principal questi logical interest before us; viz. *why* do we take average means? What is the exact nature and amount of the itage gained by so doing? The advanced student would course prefer to work out the answers to these questions appealing at once to the Law of Error in its ultimate or ponential form. But I feel convinced that the best thod for those who wish to gain a clear conception of the ical nature of the process involved, is to begin by treating as a question of combinations such as we are familiar with elementary algebra; in other words to take a finite number errors and to see what comes of averaging these. We can en proceed to work out arithmetically the results of comning two or more of the errors together so as to get a new ies, not contenting ourselves with the general character rely of the new law of error, but actually calculating what is in the given case. For the sake of simplicity we will take a series with a very large number of terms in it, but will be well to have enough of them to secure that our law error shall roughly approximate in its form to the standard exponential law.

For this purpose the law of error or divergence given by posing our effort to be affected by ten causes, each of ich produces an equal error, but which error is equally ly to be positive and negative (or, as it might perhaps expressed, 'ten equal and indifferently additive and tractive causes') will suffice. This is the lowest number med according to the Binomial law, which will furnish to eye a fair indication of the limiting or Exponential law[1]. e whole number of possible cases here is 2^{10} or 1024; that this is the number required to exhibit not only all the es which can occur (for there are but eleven really disct cases), but also the relative frequency with which each hese cases occurs in the long run. Of this total, 252 will be situated at the mean, representing the ‘true’ r or that given when five of the causes of disturbance neutralize the other five. Again, 210 will be at wha will call one unit’s distance from the mean, or that giv six causes combining against four; and so on; until a extreme distance of five places from the mean we ge one result, since in only one case out of the 1024 will a causes combine together in the same direction. The s 1024 efforts is therefore a fair representation of the d bution of an infinite number of such efforts. A grap representation of the arrangement is given here.

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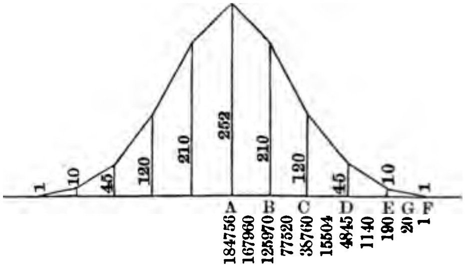
[1] See, for the explanation of this, and of the graphical method of illusing it, the note on p. 29.



§ 11. This representing a complete set of single servations or efforts, what will be the number and arr ment in the corresponding set of combined or re observations, say of two together? With regard to number we must bear in mind that this is not a case o combinations of things which cannot be repeated; for given error, say the extreme one at *F*, can obvious repeated twice running. Such a repetition would be a of very bad luck no doubt, but being possible it must its place in the set. Now the possible number of wa combining 1024 things two together, where the same may be repeated twice running, is 1024 × 1024 or 104 is then is the number in a complete cycle of the results ten two and two together.

§ 12. So much for their number; now for their arrange-ǝnt or distribution. What we have to ascertain is, firstly, w many times each possible pair of observations will present elf; and, secondly, where the new results, obtained from e combination of each pair, are to be placed. With regard the first of these enquiries;—it will be readily seen that one occasion we shall have *F* repeated twice; on 20 occa-ns we shall have *F* combined with *E* (for *F* coming first may have it followed by any one of the 10 at *E*, or any e of these may be followed by *F*); *E* can be repeated in × 10, or 100 ways, and so on.

Now for the position of each of these reduced observations, relative frequency of whose component elements has thus ǝn pointed out. This is easy to determine, for when we ce two errors there is (as was seen) scarcely any other de of treatment than that of selecting the mid-point be-een them; this mid-point of course becoming identical th each of them when the two happen to coincide. It will seen therefore that *F* will recur once on the new arrange-ǝnt, viz. by its being repeated twice on the old one. *G*, dway between *E* and *F*, will be given 20 times. *E*, on r new arrangement, can be got at in two ways, viz. by its ing repeated twice (which will happen 100 times), and by being obtained as the mid-point between *D* and *F* (which ll happen 90 times). Hence *E* will occur 190 times alto-ther.

The reader who chooses to take the trouble may work out e frequency of all possible occurrences in this way, and if e object were simply to illustrate the principle in accord-ce with which they occur, this might be the best way of oceeding. But as he may soon be able to observe, and as

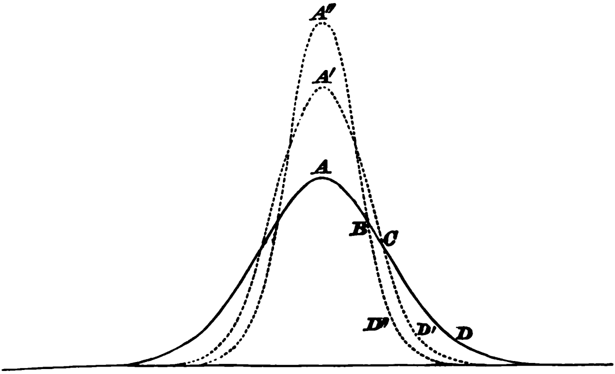
the mathematician would at once be able to prove, the
'law of facility of error' can be got at more quickly de
tively, viz. by taking the successive terms of the expansio
$(1+1)^{20}$. They are given, below the line, in the figure on p.

§ 13. There are two apparent obstacles to any di
comparison between the distribution of the old set of sim
observations, and the new set of combined or reduced o
In the first place, the number of the latter is much grea
This, however, is readily met by reducing them both to
same scale, that is by making the same total number of e
In the second place, half of the new positions have no re
sentatives amongst the old, viz. those which occur mid
between *F* and *E*, *E* and *D*, and so on. This can be me
the usual plan of interpolation, viz. by filling in such
by estimating what would have been the number at
missing points, on the same scale, had they been occu
Draw a curve through the vertices of the ordinate
A, *B*, *C*, &c., and the lengths of the ordinates at the
termediate points will very fairly represent the correspon
frequency of the errors of those magnitudes respecti
When the gaps are thus filled up, and the numbers
reduced to the same scale, we have a perfectly fair basi
comparison. (See figure on next page.)

Similarly we might proceed to group or 'reduce' t
observations, or any greater number. The number of poss
groupings naturally becomes very much larger, being (10
when they are taken three together. As soon as we ge
three or more observations, we have (as already pointed
a variety of possible modes of treatment or reduction
which that of taking the arithmetical mean is but one.

§ 14. The following figure is intended to illustrate
nature of the advantage secured by thus taking the ar
metical mean of several observations.

The curve *ABCD* represents the arrangement of a given mber of 'errors' supposed to be disposed according to the nomial law already mentioned, when the angles have been oothed off by drawing a curve through them. *A'CD'* presents the similar arrangement of the same number hen given not as simple errors, but as averages of pairs of rors. *A''BD''*, again, represents the similar arrangement tained as averages of errors taken three together. They e drawn as carefully to scale as the small size of the figure rmits.

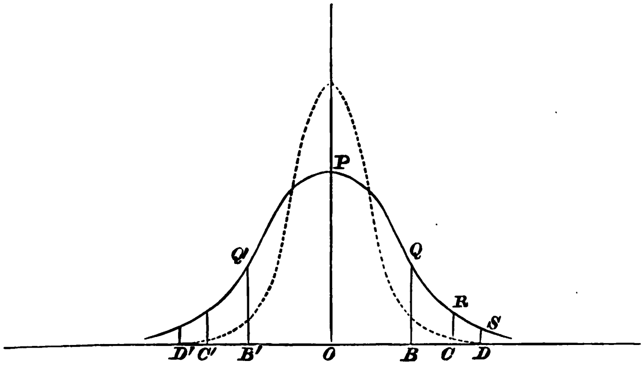


§ 15. A glance at the above figure will explain to the der, better than any verbal description, the full significance of the statement that the result of combining two or ore measurements or observations together and taking the erage of them, instead of stopping short at the single ements, is to make large errors comparatively more scarce. e advantage is of the same general description as that of hing in a lake where, of the same number of fish, there e more big and fewer little ones than in another water: of dipping in a bag where of the same number of coins are more sovereigns and fewer shillings; and so on. extreme importance, however, of obtaining a perfectly conception of the subject may render it desirable to this out a little more fully in detail.

For one thing, then, it must be clearly understood the result of a set of 'averages' of errors is nothing than another set of 'errors.' No device can make th tainment of the true result certain,—to suppose the trary would be to misconceive the very foundations of bability,—no device even can obviate the possibility of l actually worse off as the result of our labour. The av of two, three, or any larger number of single results, give a worse result, i.e. one further from the ultimate ave than was given by the first observation we made. We simply fall back upon the justification that big devia are rendered scarcer in the long run.

Again; it may be pointed out that though, in the a investigation, we have spoken only of the arithmetical av as commonly understood and employed, the same ge results would be obtained by resorting to almost any metrical and regular mode of combining our observatio errors. The two main features of the regularity displ by the Binomial Law of facility were (1) ultimate symm about the central or true result, and (2) increasing rel frequency as this centre was approached. A very little sideration will show that it is no peculiar prerogative of arithmetical mean to retain the former of these and t crease the latter. In saying this, however, a distinction be attended to for which it will be convenient to refer figure.

§ 16. Suppose that *O*, in the line *D'OD*, was the p aimed at by any series of measurements; or, what come e same thing for our present purpose, was the ultimate erage of all the measurements made. What we mean by



symmetrical arrangement of the values in regard to *O*, is at for every error *OB*, there shall be in the long run a pre-ely corresponding opposite one *OB′*; so that when we erect e ordinate *BQ*, indicating the frequency with which *B* is elded, we must erect an equal one *B′Q′*. Accordingly the o halves of the curve on each side of *P*, viz. *PQ* and *PQ′* e precisely alike.

It then readily follows that the secondary curve, viz. at marking the law of frequency of the averages of two or re simple errors, will also be symmetrical. Consider any ree points *B*, *C*, *D*: to these correspond another three , *C′*, *D′*. It is obvious therefore that any regular and sym-etrical mode of dealing with all the groups, of which *BCD* a sample, will result in symmetrical arrangement about e centre *O*. The ordinary familiar arithmetical average is t one out of many such modes. One way of describing it is saying that the average of *B*, *C*, *D*, is assigned by choosing oint such that the sum of the squares of its distances from

B, *C*, *D*, is a minimum. But we might have selected such that the cubes, or the fourth powers, or any powers should be a minimum. These would all yield resembling in a general way the dotted line in our Of course there would be insuperable practical object any such courses as these; for the labour of calc would be enormous, and the results so far from being would be worse than those afforded by the employn the ordinary average. But so far as concerns the principle of dealing with discordant and erroneous re must be remembered that the familiar average is l out of innumerable possible resources, all of which yield the same sort of help.

§ 17. Once more. We saw that a resort to the had the effect of 'humping up' our curve more towa centre, expressive of the fact that the errors of avera of a better, i.e. smaller kind. But it must be notic exactly the same characteristics will follow, as a gener from any other such mode of dealing with the ind errors. No strict proof of this fact can be given her reference to one of the familiar results of taking cc tions of things will show whence this tendency arise treme results, as yielded by an average of any kind, c be got in one way, viz. by repetitions of extremes individuals from which the averages were obtainec intermediate results can be got at in two ways, viz. ei intermediate individuals, or by combinations of indi in opposite directions. In the case of the Binomi of Error this tendency to thicken towards the cen already strongly predominant in the individual values we took them in hand for our average; but owing characteristic of combinations we may lay it down (speaking) that any sort of average applied to any sort ribution will give a result which bears the same l relation to the individual values that the dotted lines bear to the black line[1].

8. This being so, the speculative advantages of one d of combining, or averaging, or reducing, our observa- over another method,—irrespective, that is, of the al conveniences in carrying them out,—will consist in the degree of rapidity with which it tends thus to the result about the centre. We shall have to subject erit to a somewhat further analysis, but for the present e it will suffice to say that if one kind of average gave gher dotted line in the figure on p. 479 and another he lower dotted line, we should say that the former e better one. The advantage is of the same general s that which is furnished in algebraical calculation, by s which converges rapidly towards the true value as ed with one which converges slowly. We can do rk sooner or later by the aid of either; but we get the truth by the same amount of labour, or get as y a less amount of labour, on one plan than on the

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we are here considering the case in which the indi- observations are supposed to be grouped in accordance

dly speaking, we may say above remarks hold good of of frequency of error in here are actual limits, how- e, to the possible magnitude or. If there are no limits to ble errors, this characteristic erage to heap its results up the centre will depend upon ances. When, as in the ex- l curve, the approximation se, as asymptote, is exceedingly rapid,—that is, when the extreme errors are relatively very few,—it still holds good. But if we were to take as our law of facility such an equation as $y = \frac{\pi}{1+x^2}$, (as hinted by De Morgan and noted by Mr Edgeworth: *Camb. Phil. Trans.* vol. x. p. 184, and vol. XIV. p. 160) it does not hold good. The result of averaging is to *diminish* the tendency to cluster towards the centre.

with the Binomial Law, it will suffice to say that in this there is no doubt that the arithmetical average is not the simplest and easiest to deal with, but is also the be the above sense of the term. And since this Binomial or something approximating to it, is of very wide preval a strong primâ facie case is made out for the general emp ment of the familiar average.

§ 19. The analysis of a few pages back carried results of the averaging process as far as could be veniently done by the help of mere arithmetic. To further we must appeal to higher mathematics, but following indication of the sort of results obtained suffice for our present purpose. After all, the succes steps, though demanding intricate reasoning for their are nothing more than generalizations of processes could be established by simple arithmetic[1]. Briefly, what do is this:—

(1) We first extend the proof from the binomial with its finite number of elements, to the limiting ponential form. Instead of confining ourselves to a number of discrete errors, we then recognize the possibi of any number of errors of any magnitude whatever.

(2) In the next place, instead of confining ourselve the consideration of an average of two or three only already, as we have seen, a tedious piece of arithmetic.— calculate the result of an average of any number, n. actual result is extremely simple. If the modulus of single errors is c, that of the average of n of these will $c \div \sqrt{n}$.

(3) Finally we draw similar conclusions in reference the *sum or difference* of two averages of any numbers.

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[1] The reader will find the proofs in Galloway *on Probability*. of these and other similar formulæ Airy on Errors.

ose, for instance, that m errors were first taken and aver-ged, and then n similarly taken and averaged. These verages will be nearly, but not quite, equal. Their sum or ifference,—these, of course, are indistinguishable in the end, ince positive and negative errors are supposed to be equal nd opposite,—will itself be an 'error', every magnitude of hich will have a certain assignable probability or facility of ccurrence. What we do is to assign the modulus of *these* rrors. The actual result again is simple. If c had been e modulus of the single errors, that of the sum or dif-rence of the averages of m and n of them will be

$$c\sqrt{\frac{1}{m}+\frac{1}{n}}.$$

§ 20. So far, the problem under investigation has been f a direct kind. We have supposed that the ultimate mean alue or central position has been given to us; either à riori (as in many games of chance), or from more immediate hysical considerations (as in aiming at a mark), or from ex-nsive statistics (as in tables of human stature). In all ach cases therefore the main desideratum is already taken r granted, and it may reasonably be asked what remains to e done. The answers are various. For one thing we may ant to estimate the value of an average of many when com-ared with an average of a few. Suppose that one man has llected statistics including 1000 instances, and another has llected 4000 similar instances. Common sense can recog-ize that the latter are better than the former; but it has no ea *how much* better they are. Here, as elsewhere, quanti-tive precision is the privilege of science. The answer we ceive from this quarter is that, in the long run, the odulus,—and with this the probable error, the mean error, nd the error of mean square, which all vary in proportion,—

diminishes inversely as the square root of the numb measurements or observations. (This follows from the se of the above formulæ.) Accordingly the probable err the more extensive statistics here is one half that of the extensive. Take another instance. Observation shows "the mean height of 2,315 criminals differs from the r height of 8,585 members of the general adult populatio about two inches" (v. Edgeworth, Methods of Statis *Stat. Soc. Journ.* 1885). As before, common sense would little doubt that such a difference was significant, bu could give no numerical estimate of the significance. pealing to science, we see that this is an illustration of third of the above formulæ. What we really want to k is the odds against the averages of two large batches diffe by an assigned amount: in this case by an amount equa twenty-five times the modulus of the variable quan The odds against this are many billions to one.

§ 21. The number of direct problems which will admit of solution is very great, but we must confine ourse here to the main inverse problem to which the foreg discussion is a preliminary. It is this. Given *a few on* one of these groups of measurements or observations; can we do with these, in the way of determining that m about which they would ultimately be found to clus Given a large number of them, they would betray the tion of their ultimate centre with constantly increa certainty: but we are now supposing that there are on few of them at hand, say half a dozen, and that we hav power at present to add to the number.

In other words,—expressing ourselves by the aid graphical illustration, which is perhaps the best met for the novice and for the logical student,—in the di problem we merely have to draw the curve of frequency f knowledge of its determining elements; viz. the position f the centre, and the numerical value of the modulus. In he inverse problem, on the other hand, we have three eleents at least, to determine. For not only must we, (1), as efore, determine whereabouts the centre may be assumed to e; and (2), as before, determine the value of the modulus r degree of dispersion about this centre. This does not omplete our knowledge. Since neither of these two eleents is assigned with certainty, we want what is always quired in the Theory of Chances, viz. some estimate of their robable truth. That is, after making the best assignment e can as to the value of these elements, we want also to sign numerically the 'probable error' committed in such signment. Nothing more than this can be attained in Probility, but nothing less than this should be set before us.

§ 22. (1) As regards the first of these questions, the nswer is very simple. Whether the number of measureents or observations be few or many, we must make the sumption that their average *is* the point we want; that is, hat the average of the few will coincide with the ultimate verage. This is the best, in fact the only assumption we an make. We should adopt this plan, of course, in the xtreme case of there being only *one* value before us, by just king that one; and our confidence increases slowly with he number of values before us. The only difference therere here between knowledge resting upon such data, and nowledge resting upon complete data, lies not in the result btained but in the confidence with which we entertain it.

§ 23. (2) As regards the second question, viz. the deterination of the modulus or degree of dispersion about the ean, much the same may be said. That is, we adopt the ame rule for the determination of the E.M.S. (error of mean quare) by which the modulus is assigned, as we should adopt if we possessed full information. Or rather w confined to *one* of the rules given on p. 473, viz. the se for by supposition we have neither the à priori know which would be able to supply the first, nor a suff number of observations to justify the third. That i reckon the errors, measured from the average, and calc their mean square: twice this is equal to the square c modulus of the probable curve of facility[1].

§ 24. (3) The third question demands for its sol somewhat advanced mathematics; but the results ca indicated without much difficulty. A popular way of st our requirement would be to say that we want to know likely it is that the mean of the few, which we have accepted, shall coincide with the true mean. But this be to speak loosely, for the chances are of course indefi great against such precise coincidence. What we rea is to assign the 'probable error'; that is, to assign a which it is as likely as not that the discrepancy be the inferred mean and the true mean should exceed[2] take a numerical example: suppose we had made s easurements of a wall with a tape, and that the average of ese was 150 feet. The scrupulous surveyor would give us is result, with some such correction as this added,—'pro-ble error 3 inches'. All that this means is that we may ume that the true value is 150 feet, with a confidence that half the cases (of this description) in which we did so, we ould really be within three inches of the truth.

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[1] The formula commonly used for the E.M.S. in this case is $\frac{\Sigma e^2}{n-1}$ and not $\frac{\Sigma e^2}{n}$. The difference is trifling, unless n be small; the justification has been offered for it that since the sum of the squares measured from the true centre is a minimum (that centre being the ultimate arithmetical mean) the sum of the squares measured from the somewhat incorrectly assigned centre will be somewhat larger.

[2] It appears to me that in strict logical propriety we should know the probable error comm *both* the assignments of the pr two sections. But the profou thematicians who have discuss question, and who alone are tent to treat it, have mostly with the practical wants of Astr in view; and for this purpos sufficient to take account of t great desideratum, viz. the true sought. Accordingly the onl commonly given refer to the pr error of the mean.

The expression for this probable error is a simple multiple the modulus: it is the modulus multiplied by ·4769... at it should be some function of the modulus, or E.M.S., ems plausible enough; for the greater the errors,—in other rds the wider the observed discrepancy amongst our easurements,—the less must be the confidence we can feel the accuracy of our determination of the mean. But, of urse, without mathematics we should be quite unable to tempt any numerical assignment.

§ 25. The general conclusion therefore is that the de-rmination of the curve of facility,—and therefore ultimately every conclusion which rests upon a knowledge of this rve,—where only a few observations are available, is of st the same kind as where an infinity are available. The les for obtaining it are the same, but the confidence with hich it can be accepted is less.

The knowledge, therefore, obtainable by an average of a all number of measurements of any kind, hardly differs cept in degree from that which would be attainable by an definitely extensive series of them. We know the same rt of facts, only we are less certain about them. But, on e other hand, the knowledge yielded by an average even a small number differs in kind from that which is yielded a single measurement. Revert to our marksman, whose llseye is supposed to have been afterwards removed. If had fired only a single shot, not only should we be less certain of the point he had aimed at, but we should have means whatever of guessing at the quality of his shooting of inferring in consequence anything about the prob remoteness of the next shot from that which had gone bef But directly we have a plurality of shots before us, we merely feel more confident as to whereabouts the centr aim was, but we also gain some knowledge as to how future shots will cluster about the spot thus indicated. quality of his shooting begins at once to be betrayed by results.

§ 26. Thus far we have been supposing the Law Facility to be of the Binomial type. There are sev reasons for discussing this at such comparative length. one thing it is the only type which,—or something appr mately resembling which,—is actually prevalent over a range of phenomena. Then again, in spite of its appa intricacy, it is really one of the simplest to deal with; ow to the fact that every curve of facility derived from it taking averages simply repeats the same type again. curve of the average only differs from that of the sin elements in having a smaller modulus; and its modulu smaller in a ratio which is exceedingly easy to give. If t of the one is c, that of the other (derived by averag n single elements) is $\frac{c}{\sqrt{n}}$.

But for understanding the theory of averages we m consider other cases as well. Take then one which is int sically as simple as it possibly can be, viz. that in which values within certain assigned limits are equally prob This is a case familiar enough in abstract Probability, thou as just remarked, not so common in natural phenomena. is the state of things when we act at random directly u e objects of choice[1]; as when, for instance, we choose digits random out of a table of logarithms.

The reader who likes to do so can without much labour rk out the result of taking an average of two or three ults by proceeding in exactly the same way which we opted on p. 476. The 'curve of facility' with which we ve to start in this case has become of course simply a ite straight line. Treating the question as one of simple mbinations, we may divide the line into a number of equal rts, by equidistant points; and then proceed to take these o and two together in every possible way, as we did in the e discussed some pages back.

If we did so, what we should find would be this. When average of *two* is taken, the 'curve of facility' of the erage becomes a triangle with the initial straight line for e; so that the ultimate mean or central point becomes e likeliest result even with this commencement of the eraging process. If we were to take averages of three, r, and so on, what we should find would be that the nomial law begins to display itself here. The familiar bell pe of the exponential curve would be more and more sely approximated to, until we obtained something quite distinguishable from it.

§ 27. The conclusion therefore is that when we are aling with averages involving a considerable number it is t necessary, in general, to presuppose the binomial law of tribution in our original data. The law of arrangement of hat we may call the derived curve, viz. that corresponding the averages, will not be appreciably affected thereby. cordingly we seem to be justified in bringing to bear all the same apparatus of calculation as in the former cas take the initial average as the probable position of th centre or ultimate average: we estimate the probabili we are within an assignable distance of the truth in so by calculating the 'error of mean square'; and we to this same element to determine the modulus, i. amount of contraction or dispersion, of our derived c facility.

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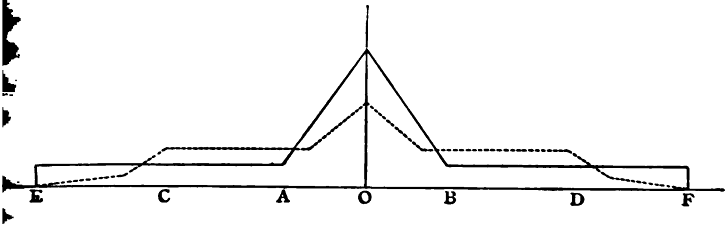
[1] i.e. as distinguished from acting on them indirectly. This latter ceeding, as explained in the chapter on Randomness, may result in giving a non-uniform distribution.

The same general considerations will apply to mos kinds of Law of Facility. Broadly speaking,—we sha to the examination of certain exceptions immedia whatever may have been the primitive arrangemen that of the single results) the arrangement of the d results (i.e. that of the averages) will be more crowd towards the centre. This follows from the character combinations already noticed, viz. that extreme valu only be got at by a repetition of several extremes, w intermediate values can be got at either by repetit intermediates or through the counteraction of opposi tremes. Provided the original distribution be symm about the centre, and provided the limits of possible e finite, or if infinite, that the falling off of frequency recede from the mean be very rapid, then the resu taking averages will be better than those of trusti single results.

§ 28. We will now take notice of an exceptional We shall do so, not because it is one which can actually occur, but because the consideration of it will us to ask ourselves with some minuteness what we m the above instances by calling the results of the av 'better' than those of the individual values. A diagra bring home to us the point of the difficulty better tha verbal or symbolic description.

The black line represents a Law of Error easily stated in words, and one which, as we shall subsequently see, can be



conceived as occurring in practice. It represents a state of things under which up to a certain distance from *O*, on each side, viz. to *A* and *B*, the probability of an error diminishes uniformly with the distance from *O*; whilst beyond these points, up to *E* and *F*, the probability of error remains constant. The dotted line represents the resultant Law of Error obtained by taking the average of the former two and two together. Now is the latter 'better' than the former? Under it, certainly, great errors are less frequent and intermediate ones more frequent; but then on the other hand the *small* errors are less frequent: is this state of things on the whole an improvement or not? This requires us to reconsider the whole question.

§ 29. In all the cases discussed in the previous sections the superiority of the curve of averages over that of the single results showed itself at every point. The big errors were scarcer and the small errors were commoner; it was only just at one intermediate point that the two were on terms of equality, and this point was not supposed to possess any particular significance or importance. Accordingly we had no occasion to analyse the various cases included under the general relation. It was enough to say that one was better than the other, and it was sufficient for all purposes to take the 'modulus' as the measure of this superiori fact we are quite safe in simply saying that the *ave* those average results is better than that of the ind ones.

When however we proceed in what Hume call sifting humour," and enquire *why* it is sufficient t trust to the average; we find, in addition to the con tions hitherto advanced, that some postulate was requ to the *consequences* of the errors we incur. It invol estimate of what is sometimes called the 'detriment' error. It seemed to take for granted that large and errors all stand upon the same general footing of bein chievous in their consequences, but that their evil effe crease in a greater ratio than that of their own magnit

§ 30. Suppose, for comparison, a case in which t portance of an error is directly proportional to its mag (of course we suppose positive and negative errors to b each other in the long run): it does not appear th advantage would be gained by taking averages. Som of this sort may be considered to prevail in cases o purchase and sale. Suppose that any one had to buy large number of yards of cloth at a constant price per that he had to do this, say, five times a day for many d succession. And conceive that the measurement o cloth was roughly estimated on each separate occasion resultant errors which are as likely to be in excess defect. Would it make the slightest difference t whether he paid separately for each piece; or w the five estimated lengths were added together, their a taken, and he were charged with this average price fo piece? In the latter case the errors which will be ma the estimation of each piece will of course be less in th run than they would be in the former: will this be o nsequence? The answer surely is that it will not make e slightest difference to either party in the bargain. In e long run, since the same parties are concerned, it will not atter whether the intermediate errors have been small or rge.

Of course nothing of this sort can be regarded as the neral rule. In almost every case in which we have to ake measurements we shall find that large errors are much ore mischievous than small ones, that is, mischievous in a eater ratio than that of their mere magnitude. Even in rchase and sale, where *different* purchasers are concerned, is must be so, for the pleasure of him who is overserved ill hardly equal the pain of him who is underserved. And many cases of scientific measurement large errors may be mply fatal, in the sense that if there were no reasonable ospect of avoiding them we should not care to undertake e measurement at all.

§ 31. If we were only concerned with practical con- derations we might stop at this point; but if we want to alize the full logical import of average-taking as a means this particular end, viz. of estimating some assigned agnitude, we must look more closely into such an ex- eptional case as that which was indicated in the figure on 493. What we there assumed was a state of things in eference to which extremely small errors were very fre- uent, but that when once we got beyond a certain small nge all other errors, within considerable limits, were equally kely.

It is not difficult to imagine an example which will aptly lustrate the case in point: at worst it may seem a little far- etched. Conceive then that some firm in England received hurried order to supply a portion of a machine, say a team-engine, to customers at a distant place; and that it was absolutely essential that the work should be true tenth of an inch for it to be of any use. But concei that two specifications had been sent, resting on di measurements, in one of which the length of the re piece was described as sixty and in the other six inches. On the assumption of any ordinary law of whether of the binomial type or not, there can be no that the firm would make the best of a very bad job b structing a piece of 60 inches and a half: i.e. they have a better chance of being within the requisite te an inch by so doing, than by taking either of the two s cations at random and constructing it accurately tc But if the law were of the kind indicated in our dia then it seems equally certain that they would be less to be within the requisite narrow margin by so doing. mere question of probability,—that is, if such estimate acted upon again and again,—there would be fewer f encountered by simply choosing one of the conf measurements at random and working exactly to this by trusting to the average of the two.

This suggests some further reflections as to the tak averages. We will turn now to another exceptional but one involving somewhat different considerations those which have been just discussed. As before, it n most conveniently introduced by commencing with a ample.

---

[1] There is no difficulty in conceiving circumstances under which a law very closely resembling this would prevail. Suppose, e.g., that one of the two measurements had been made by a careful and skilled mechanic, and the other by a man who to save himself trouble had put in the estimate at random certain limits),—the firm h knowledge of this fact but t course unable to assign the their authors,—we should g much such a Law of Error as posed above.

§ 32. Suppose then that two scouts were sent to take e calibre of a gun in a hostile fort,—we may conceive that e fort was to be occupied next day, and used against the emy, and that it was important to have a supply of shot or ll,—and that the result is that one of them reports the libre to be 8 inches and the other 9. Would it be wise to ume that the mean of these two, viz. $8\frac{1}{2}$ inches, was a elier value than either separately?

The answer seems to be this. If we have reason to ppose that the possible calibres partake of the nature of a *tinuous* magnitude,—i.e. that all values, with certain its, are to be considered as admissible, (an assumption ich we always make in our ordinary inverse step from an ervation or magnitude to the thing observed or measured) then we should be justified in selecting the average as the elier value. But if, on the other hand, we had reason to ppose that *whole* inches are always or generally preferred, is in fact the case now with heavy guns, we should do tter to take, even at hazard, one of the two estimates set fore us, and trust this alone instead of taking an average the two.

§ 33. The principle upon which we act here may be ted thus. Just as in the direct process of calculating or playing the 'errors', whether in an algebraic formula or in diagram, we generally assume that their possibility is ntinuous, i.e. that all intermediate values are possible; so, the inverse process of determining the probable position of e original from the known value of two or more errors, we ume that that position is capable of falling at any point hatever between certain limits. In such an example as the ove, where we know or suspect a discontinuity of that ssibility of position, the value of the average may be tirely destroyed.

In the above example we were supposed to know the calibre of the guns was likely to run in English inc in some other recognized units. But if the battery we China or Japan, and we knew nothing of the standar length in use there, we could no longer appeal to principle. It is doubtless highly probable that those cal are not of the nature of continuously varying magnit but in an entire ignorance of the standards actually ado we are to all intents and purposes in the same position they were of that continuous nature. When this is s objections to trusting to the average would no longer good, and if we had only one opportunity, or a very opportunities, we should do best to adhere to the custo practice.

§ 34. When however we are able to collect and com a large number of measurements of various objects, consideration of the probable discontinuity of the object thus measure,—that is, their tendency to assume some o other of a finite number of distinct magnitudes, instea showing an equal readiness to adapt themselves to all i mediate values,—again assumes importance. In fact, g a sufficient number of measurable objects, we can act deduce with much probability the standard accordin which the things in question were made.

This is the problem which Mr Flinders Petrie has tacked with so much acuteness and industry in his work *Inductive Metrology*, a work which, merely on the groun its speculative interest, may well be commended to student of Probability. The main principles on which reasoning is based are these two:—(1) that all artificers prone to construct their works according to round num or simple fractions, of their units of measurement; and that, aiming to secure this, they will stray from it in to e accordance with the law of error. The result of these o assumptions is that if we collect a very large number of asurements of the different parts and proportions of some cient building,—say an Egyptian temple,—whilst no as- nable length is likely to be permanently unrepresented, we find a marked tendency for the measurements to ster about certain determinate points in our own, or any er standard scale of measurement. These points mark the gth of the standard, or of some multiple or submultiple of standard, employed by the old builders. It need hardly said that there are a multitude of practical considerations be taken into account before this method can be expected give trustworthy results, but the leading principles upon ich it rests are comparatively simple.

§ 35. The case just considered is really nothing else n the recurrence, under a different application, of one ich occupied our attention at a very early stage. We ticed (Chap. II.) the possibility of a curve of facility which stead of having a single vertex like that corresponding to common law of error, should display two humps or rtices. It can readily be shown that this problem of the asurements of ancient buildings, is nothing more than the opening of the same question, in a slightly more complex m, in reference to the question of the functions of an erage.

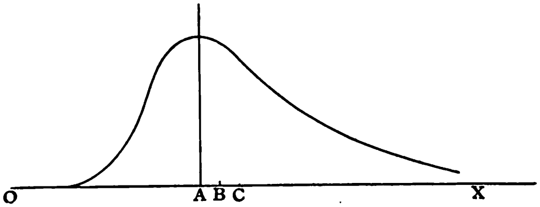
Take a simple example. Suppose an instance in which eat errors, of a certain approximate magnitude, are dis- ctly more likely to be committed than small ones, so that e curve of facility, instead of rising into one peak towards e centre, as in that of the familiar law of error, shows a pression or valley there. Imagine, in fact, two binomial rves, with a short interval between their centres. Now if were to calculate the result of taking averages here we should find that this at once tends to fill up the valley if we went on long enough, that is, if we kept on t averages of sufficiently large numbers, a peak would be arise in the centre. In fact the familiar single bin curve would begin to make its appearance.

§ 36. The question then at once suggests itself, oug to do this? Shall we give the average free play to pe its allotted function of thus crowding things up towar centre? To answer this question we must introduce tinction. If that peculiar double-peaked curve had be it conceivably might, a true error-curve,—that is, if i represented the divergences actually made in aiming real centre,—the result would be just what we should It would furnish an instance of the advantages to be g by taking averages even in circumstances which were o ally unfavourable. It is not difficult to suggest an a priate illustration. Suppose a man firing at a mark some sheltered spot, but such that the range crossed a exposed valley up or down which a strong wind was gen blowing. If the shotmarks were observed we should them clustering about two centres to the right and l the bullseye. And if the results were plotted out in a they would yield such a double-peaked curve as we described. But if the winds were equally strong and p lent in opposite directions, we should find that the aver process redressed the consequent disturbance.

If however the curve represented, as it is decidedly likely to do, some outcome of natural phenomena in there was, so to say, a real double aim on the part of n it would be otherwise. Take, for instance, the resu measuring a large number of people who belonged t very heterogeneous races. The curve of facility would be of the kind indicated on p. 45, and if the numbers wo commingled races were equal it would display a pair of win peaks. Again the question arises, 'ought' we to inolve the whole range within the scope of a single average? he answer is that the obligation depends upon the purpose e have in view. If we want to compare that heterogeneous ace, as a whole, with some other, or with itself at some ther time, we shall do well to average without analysis. ll statistics of population, as we have already seen (v. p. 47), re forced to neglect a multitude of discriminating characeristics of the kind in question. But if our object were to terpret the causes of this abnormal error-curve we should o well to break up the statistics into corresponding parts, nd subject these to analysis separately.

Similarly with the measurements of the ancient buildings. n this case if all our various 'errors' were thrown together nto one group of statistics we should find that the resultant urve of facility displayed, not two peaks only, but a succession of them; and these of various magnitudes, correponding to the frequency of occurrence of each particular easurement. We *might* take an average of the whole, but ardly any rational purpose could be subserved in so doing; hereas each separate point of maximum frequency of occurrence has something significant to teach us.

§ 37. One other peculiar case may be noticed in conlusion. Suppose a distinctly asymmetrical, or lopsided curve f facility, such as this:—



Laws of error, of which this is a graphical representat are, I apprehend, far from uncommon. The curve in quest is, in fact, but a slight exaggeration of that of barometı heights as referred to in the last chapter; when it explained that in such cases the mean, the median, and maximum ordinate would show a mutual divergence. doubt here is not, as in the preceding instances, whethe not a single average should be taken, but rather what l of average should be selected. As before, the answer n depend upon the special purpose we have in view. For ordinary purposes of comparison between one time or p and another, any average will answer, and we should th fore naturally take the arithmetical, as the most familia the median, as the simplest.

§ 38. Cases might however arise under which o kinds of average could justify themselves, with a momen notice of which we may now conclude. Suppose, for insta that the question involved here were one of desirabilit climate. The ordinary mean, depending as it does so lar upon the number and magnitude of extreme values, m very reasonably be considered a less appropriate test t that of judging simply by the relatively most frequent va in other words, by the maximum ordinate. And var other points of view can be suggested in respect of wl this particular value would be the most suitable and nificant.

In the foregoing case, viz. that of the weather cı there was no objective or 'true' value aimed at. Bı curve closely resembling this would be representativ that particular class of estimates indicated by Mr Ga and for which, as he has pointed out, the *geometrical* n becomes the only appropriate one. In this case the cur facility ends abruptly at O: it resembles a much foreshort odification of the common exponential form. Its charac-ristics have been discussed in the paper by Dr Macalister ready referred to, but any attempt to examine its properties re would lead us into far too intricate details.

§ 39. The general conclusion from all this seems quite accordance with the nature and functions of an average as inted out in the last chapter. Every average, it was urged, but a single representative intermediate value substituted a plurality of actual values. It must accordingly let slip the lk of the information involved in these latter. Occasionally, in most ordinary measurements, the one thing which it presents is obviously the thing we are in want of; and en the only question can be, which mean will most accord ith the 'true' value we are seeking. But when, as may ppen in most of the common applications of statistics, ere is really no 'true value' of an objective kind behind e phenomena, the problem may branch out in various rections. We may have a variety of purposes to work out, d these may demand some discrimination as regards the erage most appropriate for them. Whenever therefore we ve any doubt whether the familiar arithmetical average is itable for the purpose in hand we must first decide pre-isely what that purpose is.

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